

Digitized by the Internet Archive
in 2009 with funding from
University of Toronto

THE
LONDON, EDINBURGH, AND DUBLIN
PHILOSOPHICAL MAGAZINE
AND
JOURNAL OF SCIENCE.

CONDUCTED BY

SIR ROBERT KANE, LL.D. F.R.S. M.R.I.A. F.C.S.

SIR WILLIAM THOMSON, KNT. LL.D. F.R.S. &c.

AND

WILLIAM FRANCIS, PH.D. F.L.S. F.R.A.S. F.C.S.

“Nec araneorum sane textus ideo melior quia ex se fila gignunt, nec noster vilior quia ex alienis libamus ut apes.” JUST. LIPS. *Polit. lib. i. cap. 1.* Not.

VOL. XXVIII.—FIFTH SERIES.

JULY—DECEMBER 1889.

LONDON:

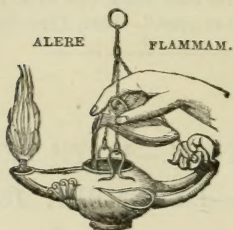
TAYLOR AND FRANCIS, RED LION COURT, FLEET STREET.

SOLD BY KENT AND CO.; SIMPKIN, MARSHALL, AND CO.; AND WHITTAKER AND CO.;—
AND BY ADAM AND CHARLES BLACK, AND T. AND T. CLARK, EDINBURGH;
SMITH AND SON, GLASGOW;—HODGES, FIGGIS, AND CO.,
DUBLIN;—PUTNAM, NEW YORK;—VEUVE J. ROYVEAU,
PARIS;—AND ASHER AND CO., BERLIN.

"Meditationis est perscrutari occulta; contemplationis est admirari perspicua Admiratio generat quæstionem, quæstio investigationem, investigatio inventionem."—*Hugo de S. Victore*.

—“Cur spirent venti, cur terra dehiscat,
Cur mare turgescat, pelago cur tantus amaror,
Cur caput obscura Phœbus ferrugine condât,
Quid toties diros cogat flagrare cometas,
Quid pariat nubes, veniant cur fulmina cœlo,
Quo micet igne Iris, superos quis conciat orbes
Tam vario motu.”

J. B. Pinelli ad Mazonium.



QC
1
P4
ser. 5
v. 28

18066
13/11/91

6.

CONTENTS OF VOL. XXVIII.

(FIFTH SERIES).

NUMBER CLXX.—JULY 1889.

	Page
Mr. J. J. Thomson on the Magnetic Effects produced by Motion in the Electric Field	1
Prof. C. V. Boys on Quartz as an Insulator. (Plate I.)	14
Messrs. Cary T. Hutchinson and Gilbert Wilkes on a Comparison of the Mercury Unit with the British Association Unit of Resistance	17
Svante Arrhenius on Electrolytic Dissociation <i>versus</i> Hydration.	30
Dr. J. H. Gladstone and Mr. Walter Hibbert on the Molecular Weight of Caoutchouc and other Colloid Bodies	38
Mr. George Fuller on a Water-spray Influence-Machine.....	42
Prof. Oliver J. Lodge and Dr. J. L. Howard on Electric Radiation and its Concentration by Lenses	48
Notices respecting New Books:—	
Dr. J. Croll's Stellar Evolution and its Relations to Geological Time	66
Mr. E. A. Ridsdale's Cosmic Evolution.....	67
Proceedings of the Geological Society:—	
Dr. C. Callaway on the Production of Secondary Minerals at Shear-zones in the Crystalline Rocks of the Malvern Hills.....	67
Messrs. G. A. J. Cole and A. V. Jennings on the Northern Slopes of Cader Idris.....	69
Rev. E. Hill on the Rocks of Alderney and the Casquets..	70
The late Mr. A. Champernowne on the Ashprington Volcanic Series of South Devon	71
On a possible Geological Origin of Terrestrial Magnetism, by Prof. Edward Hull	71
Notes on Metallic Spectra, by C. C. Hutchins.....	73
On the Influence of Solar Radiation on the Electrical Phenomena in the Atmosphere of the Earth, by Sv. Arrhenius ..	75

NUMBER CLXXI.—AUGUST.

	Page
Lord Rayleigh on Achromatic Interference-Bands	77
Mr. A. W. Clayden on some Photographs of Lightning and of "Black" Electric Sparks	92
Mr. J. T. Bottomley on Expansion with Rise of Temperature of Wires under Pulling Stress. (Plate IX.)	94
Messrs. Louis Duncan, Gilbert Wilkes, and Cary T. Hutchinson on a Determination of the Value of the B.A. Unit of Resistance in Absolute Measure, by the Method of Lorenz.	98
Dr. H. H. Hoffert on Intermittent Lightning-Flashes. (Plate IV.)	106
Mr. R. T. Glazebrook on the Reflexion and Refraction of Light at the Surface of a Crystal on the Quasi-labile Æther Theory	110
Prof. H. Hertz on the Propagation of Electric Waves through Wires	117
Prof. R. J. Anderson on an Apparatus Illustrating Crystal Forms. (Plate II.)	127
Mr. E. W. Smith on a Shunt-Transformer	132
Mr. A. W. Ward on the Use of the Biquartz in determining the position of the Plane of Polarization	134
Notices respecting New Books :—	
Dr. J. Casey's Treatise on Spherical Trigonometry, and its application to Geodesy and Astronomy	138
Proceedings of the Geological Society :—	
Major-Gen. C. A. McMahon on the Hornblende Schists and Banded Crystalline Rocks of the Lizard	139
Mr. T. Roberts on the Upper Jurassic Clays of Lincoln- shire	140
Mr. J. R. Kilroe on the Origin of Movements in the Earth's Crust	141
Prof. J. Prestwich on the Relation of the Westleton Beds or Pebbly Sands of Suffolk to those of Norfolk	142
Mr. F. Rutley on Tachylyte from Victoria Park, White- inch, near Glasgow	143
Mr. H. G. Lyons on the Bagshot Beds and their Strati- graphy	144
On the Kinetics of Bodies in Solution, by W. Nernst	145
On the Molecular Condition of Dissolved Iodine, by Morris Loeb	146
Some Observations on the Passage of Electricity through Gases and Vapours, by Dr. Natterer	147
On the Electrical Resistance of Insulators at High Tempera- ture, by Dr. H. Koller	147
On the Resistance to Disruptive Discharge offered by Gases under High Pressures, by Max Wolf	148
The Nature of Solutions, by Prof. S. U. Pickering	148

NUMBER CLXXII.—SEPTEMBER.

	Page
Prof. E. Wiedemann on the Mechanics of Luminosity. (Plate III.)	149
Messrs. J. T. Bottomley and A. Tanakadaté on the Thermo- electric Position of Platinoid	163
Mr. Charles Tomlinson on the Theory of Hail. (Plate V.)..	169
Dr. Charles V. Burton on Endless Availability: and on a Restriction to the Application of Carnot's Principle	185
Lord Rayleigh on Achromatic Interference-Bands.....	189
Mr. A. Tanakadaté on the Thermal Effect due to Reversals of Magnetization in Soft Iron. (Plates VI.—VIII.).....	207
Notices respecting New Books:—	
Prof. R. H. Smith's Graphics, or the Art of Calculation by Drawing Lines, applied especially to Mechanical Engineering. Part I.	218
On Leidenfrost's Phenomenon, by K. S. Kristensen.....	220
Determination of the Change which the Internal Friction of Air experiences at different Temperatures and under Various Pressures, by P. De Heen	220

NUMBER CLXXIII.—OCTOBER.

Mr. R. F. D'Arcy on the Viscosity of Solutions. (Plate XI.)	221
Prof. Silvanus P. Thompson's Notes on Geometrical Optics. —Part I.	232
Prof. E. Wiedemann on the Mechanics of Luminosity	248
Rev. T. Pelham Dale on a Relation existing between the Density and Refraction of Gaseous Elements, and also of some of their Compounds; with a Note by Prof. Rücker..	263
Mr. James C. McConnel on Diffraction-Colours, with special reference to Coronæ and Iridescent Clouds. (Plate X.) ..	272
Dr. G. Gore on the Molecular Constitution of Isomeric Solutions &c.	289
Prof. H. A. Rowland on the Ratio of the Electrostatic to the Electromagnetic Units of Electricity	304
Mr. Edward B. Rosa on the Determination of v , the Ratio of the Electromagnetic to the Electrostatic Unit	315
Dr. Edmond van Aubel on the Electrical Resistance of Bismuth	332
Notices respecting New Books:—	
Mr. Grant Allen's Force and Energy. A Theory of Dynamics	349
On the Dependence of the Electromotive Position of Pal- ladium on the Quantity of Hydrogen it contains, by Max Thoma	351

	Page
The Energy of Thermal Radiation at a White Heat, by O. Tumlriz and A. Krug	352
Calculation of the Mechanical Equivalent of Light from the Experiments of Julius Thomsen, by O. Tumlriz.....	352

NUMBER CLXXIV.—NOVEMBER.

Prof. R. Threlfall and Mr. A. Pollock on the Clark Cell as a Source of small Constant Currents. (Plates XII. & XIII.)	353
Mr. C. Tomlinson on Lightning and Gunpowder Magazines.	368
Prof. E. Wiedemann on the Mechanics of Luminosity.....	376
Mr. Magnus Maclean on Elementary Nomenclature in Geometrical Optics.....	400
Mr. S. A. Sworn on the Constitution of the Aromatic Nucleus.	402
Prof. Richard Threlfall on the Application of the Clark Cell to the Construction of a Standard Galvanometer. (Plate XIV. figs. 1 & 2).....	416
Mr. H. S. Carhart on an Improved Standard Clark Cell with Low Temperature-Coefficient	420
Notices respecting New Books:—	
Watts's Dictionary of Chemistry, Vol. II.	423
Dr. A. Bernthsen's Text-Book of Organic Chemistry....	424
Rev. Isaac Warren's Elementary Treatise of Mechanics.	425
On a Relation between the Sun-spot Period and the Planetary Elements, by Charles Davison	426
On the Leakage of Negative Electricity caused by Sun- and Daylight, by T. Elster and H. Geitel	427
On the Phosphorescence of Copper, Bismuth, and Manganese in the Sulphides of the Alkaline Earths, by V. Klatt and Philipp Lenard	428
On Steatite as a Source of Electricity, by M. Mentzner	428

NUMBER CLXXV.—DECEMBER.

Mr. Charles H. Lees on the Law of Cooling, and its bearing on certain Equations in the Analytical Theory of Heat	429
Mr. S. A. Sworn on the Constitution of the Aromatic Nucleus	443
Prof. R. Threlfall on the Measurement of High Specific Resistances (Plate XIV. fig. 3)	452
Prof. Richard Threlfall and Mr. A. Pollock on Measurements of the Resistance of Imperfectly Purified Sulphur..	469

	Page
B. Walter on the Relation between Fluorescence and Arrangement of Molecules	473
Dr. C. V. Burton on a Physical Basis for the Theory of Errors	480
Mr. W. G. Gregory on a Method of Driving Tuning-Forks Electrically	490
Proceedings of the Geological Society:—	
Mr. J. C. B. Hendy on a “Dumb Fault” or “Wash-out” found in the Pleasley and Teversall Collieries, Derbyshire.....	493
On the Second Law of Thermodynamics, by Prof. Eilhard Wiedemann	493
On the Maximum Galvanic Polarization of Platinum Electrodes in Sulphuric Acid, by Carl Fromme	495
Index.....	497

PLATES.

- I. Illustrative of Mr. C. V. Boys's Paper on Quartz as an Insulator.
- II. Illustrative of Prof. R. J. Anderson's Paper on an Apparatus Illustrating Crystal Forms.
- III. Illustrative of Prof. E. Wiedemann's Paper on the Mechanics of Luminosity.
- IV. Illustrative of Dr. H. H. Hoffert's Paper on Intermittent Lightning-Flashes.
- V. Illustrative of Mr. Charles Tomlinson's Paper on the Theory of Hail.
- VI.-VIII. Illustrative of Mr. Tanakadaté's Paper on the Thermal Effect due to Reversals of Magnetization in Soft Iron.
- IX. Illustrative of Mr. J. T. Bottomley's Paper on Expansion with Rise of Temperature of Wires under Pulling Stress.
- X. Illustrative of Mr. J. C. McConnel's Paper on Diffraction-Colours, with special reference to Coronæ and Iridescent Clouds.
- XI. Illustrative of Mr. R. F. D'Arcy's Paper on the Viscosity of Solutions.
- XII.-XIII. Illustrative of Prof. R. Threlfall's Paper on the Clark Cell as a Source of small Constant Currents.
- XIV. Illustrative of Prof. R. Threlfall's Papers on the Application of the Clark Cell to the Construction of a Standard Galvanometer, and on the Measurement of High Specific Resistances.

THE
LONDON, EDINBURGH, AND DUBLIN
PHILOSOPHICAL MAGAZINE
AND
JOURNAL OF SCIENCE.

[FIFTH SERIES.]

JULY 1889.

I. *On the Magnetic Effects produced by Motion in the Electric Field.* By J. J. THOMSON, M.A., F.R.S.*

IN the April number of the Philosophical Magazine Mr. Heaviside discusses the question of a moving electrified sphere, and while agreeing with the results I obtained some time ago as to the magnetic force produced by such a sphere when moving slowly, differs as to the numerical magnitude of the energy possessed by the sphere and the forces acting upon it when placed in a magnetic field. The latter quantities, however, do not depend merely upon the alterations caused by the motion of the sphere in the polarization of the dielectric surrounding the sphere, but also upon the boundary conditions we adopt and upon the view we take of the motion close to the sphere of the medium in which the electric displacements occur.

This will be seen in the course of the following investigation, in which I have endeavoured to take into account the motion of the medium in which the displacements occur. I find that, in order to close the circuits in this case, it is necessary to assume effects which, as far as I know, have not been noticed.

Let us consider the case when the electric field is that due to a charged sphere moving parallel to the axis of z with the velocity w_0 , the components at (x, y, z) of the velocity of the medium being u, v, w ; then, if we supposed that the dis-

* Communicated by the Author.

placement-currents are due entirely to variations in the electric displacement (f, g, h) caused by the motion of the sphere and the medium, the components ξ_1, η_1, ζ_1 of these currents would be given by

$$\xi_1 = u \frac{df}{dx} + v \frac{df}{dy} + (w - w_0) \frac{df}{dz},$$

$$\eta_1 = u \frac{dg}{dx} + v \frac{dg}{dy} + (w - w_0) \frac{dg}{dz},$$

$$\zeta_1 = u \frac{dh}{dx} + v \frac{dh}{dy} + (w - w_0) \frac{dh}{dz}.$$

These values, however, do not satisfy the equation

$$\frac{d\xi_1}{dx} + \frac{d\eta_1}{dy} + \frac{d\zeta_1}{dz} = 0,$$

unless the dielectric is moving uniformly; so that, if the circuits are to be closed, the motion of the medium must produce some other effect analogous to a current.

Since

$$\begin{aligned} \frac{d\xi_1}{dx} + \frac{d\eta_1}{dy} + \frac{d\zeta_1}{dz} &= \frac{d}{dx} \left(f \frac{du}{dx} + g \frac{du}{dy} + h \frac{du}{dz} \right) \\ &+ \frac{d}{dy} \left(f \frac{dv}{dx} + g \frac{dv}{dy} + h \frac{dv}{dz} \right) + \frac{d}{dz} \left(f \frac{dw}{dx} + g \frac{dw}{dy} + h \frac{dw}{dz} \right), \end{aligned}$$

we see that the currents will be closed if we add on to the components ξ_1, η_1, ζ_1 the components ξ_0, η_0, ζ_0 , where

$$-\xi_0 = f \frac{du}{dx} + g \frac{du}{dy} + h \frac{du}{dz},$$

$$-\eta_0 = f \frac{dv}{dx} + g \frac{dv}{dy} + h \frac{dv}{dz},$$

$$-\zeta_0 = f \frac{dw}{dx} + g \frac{dw}{dy} + h \frac{dw}{dz}.$$

The medium is assumed to be incompressible, so that

$$\frac{du}{dx} + \frac{dv}{dy} + \frac{dw}{dz} = 0.$$

Hence the components of the total effective currents are

$$\begin{aligned}\xi + \xi_0 &= \frac{d}{dy}(vf - ug) - \frac{d}{dz}(uh - (w - w_0)f), \\ \eta + \eta_0 &= \frac{d}{dz}((w - w_0)g - vh) - \frac{d}{dx}(vf - ug), \\ \zeta + \zeta_0 &= \frac{d}{dx}(uh - (w - w_0)f) - \frac{d}{dy}((w - w_0)g - vh).\end{aligned}$$

If the motion of the medium is irrotational, these conditions will be satisfied if we suppose that the motion of the dielectric gives rise to magnetic forces whose components α, β, γ are given by the equations

$$\left. \begin{aligned}\alpha &= 4\pi((w - w_0)g - vh), \\ \beta &= 4\pi(uh - (w - w_0)f), \\ \gamma &= 4\pi(vf - ug).\end{aligned} \right\} \quad \dots \quad (1)$$

If we suppose that the electric field is due to a number of charged spheres moving with velocities (u_1, v_1, w_1) (u_2, v_2, w_2) respectively, and producing electric displacements whose components are (f_1, g_1, h_1) (f_2, g_2, h_2) , the component of the magnetic force parallel to x will be

$$4\pi(wg - vh - \{w_1g_1 + w_2g_2 + \dots - v_1h_1 - v_2h_2 \dots\}),$$

where f, g, h are the resultant displacements.

Thus, since in the general case when the æther is in motion the assumption that the currents are merely due to the changes in the polarization caused by the æther moving from a place where the displacement has one value to another where it has a different one is insufficient if the circuits are closed, it is necessary to replace it by another; the assumption we shall adopt is that the motion of the polarized æther sets up magnetic forces whose components are given by equations (1).

When the æther is at rest this agrees with Maxwell's principle that the currents are equal to the rate of increase of the electric displacement. We should get these magnetic forces if, in the expression for the mean Lagrangian function of unit volume of the moving æther, there was the term

$$\begin{aligned}a\{wg - vh - \Sigma(w_1g_1 - v_1h_1)\} + b\{uh - vf - \Sigma(u_1h_1 - w_1f_1)\} \\ + c\{vf - ug - \Sigma(v_1f_1 - u_1g_1)\},\end{aligned}$$

where a, b, c are the components of the magnetic induction.

This term would show that there is an electromotive force parallel to x equal to

$$cv - bw,$$

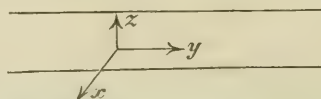
and a mechanical force equal to

$$c \frac{dg}{dt} - b \frac{dh}{dt} + g \frac{dc}{dt} - h \frac{db}{dt},$$

if the electrified bodies are at rest.

The first of these corresponds to the well-known expression for the electromotive force on a conductor moving in a magnetic field; the second is the mechanical force on a current in a magnetic field *plus* the term $g \frac{dc}{dt} - h \frac{db}{dt}$.

We can deduce an important consequence of the assumption, if we consider the case of the æther moving with uniform velocity between two parallel planes charged, the one with positive, the other with negative electricity.



If v is the velocity of the æther, h the electric displacement at right angles to the planes, the magnetic force between the planes will be parallel to x , and equal to $-4\pi v h$; or if σ is the surface-density of the electrification on the planes $-4\pi v \sigma$, the magnetic force vanishes except between the planes, so that on crossing the positively electrified surface there is an increase in the magnetic force parallel to x equal to $4\pi \sigma v$. Thus the charged surface acts like a current sheet of intensity $-\sigma v$, but $-v$ is the velocity of the plane relatively to the æther; so that a charged surface moving with velocity v relatively to the æther must act like a current sheet of intensity σv .

We will now proceed to apply these results to some special cases. Let us suppose that we have a charged sphere moving along the axis of z with the velocity w_0 , and that it sets the æther around it in motion in the same way as an incompressible fluid is set in motion by a solid sphere of the same radius moving through it with the same velocity. If a is the radius of the sphere,

$$u = \frac{1}{2} w_0 a^3 \frac{d^2}{dx dz} \frac{1}{r},$$

$$v = \frac{1}{2} w_0 a^3 \frac{d^2}{dy dz} \frac{1}{r},$$

$$w = \frac{1}{2} w_0 a^3 \frac{d^2}{dz^2} \frac{1}{r},$$

$$f = -\frac{e}{4\pi} \frac{d}{dx} \frac{1}{r}, \quad g = -\frac{e}{4\pi} \frac{d}{dy} \frac{1}{r}, \quad h = -\frac{e}{4\pi} \frac{d}{dz} \frac{1}{r} :$$

hence by equations (1),

$$\alpha = ew_0 \left(1 + \frac{1}{2} \frac{a^3}{r^3}\right) \frac{y}{r^3},$$

$$\beta = -ew_0 \left(1 + \frac{1}{2} \frac{a^3}{r^3}\right) \frac{x}{r^3},$$

$$\gamma = 0.$$

Thus the lines of magnetic force are circles with their centres along and their planes at right angles to the axis of z .

At a distance from the centre large compared with the radius of the sphere the magnetic force is the same as that due to a current ew_0 , but close to the sphere the relative motion of the sphere and æther causes it to be larger than this, and at the surface of the sphere it is the same as that due to a current $\frac{3}{2} ew_0$.

The energy due to this distribution of currents is $\frac{3}{4} \frac{e^2 w_0^2}{a} \mu$.

Another case which can be easily solved is that of a right circular cylinder rotating with an angular velocity ω , each unit length of the cylinder being charged with E units of electricity. If a is the radius of the cylinder,

$$u = -\omega \frac{a^2 y}{r^2}, \quad v = \omega \frac{a^2 x}{r^2},$$

$$f = \frac{E}{2\pi} \frac{x}{r^2}, \quad g = \frac{E}{2\pi} \frac{y}{r^2} :$$

and by equations (1),

$$\alpha = 0, \quad \beta = 0, \quad \gamma = -2E\omega \frac{a^2}{r^2}.$$

Thus outside the rotating cylinder there is a magnetic force parallel to the axis of rotation.

If we assume that the æther outside the sphere is at rest, we can find the solution of the case of a charged metal sphere executing harmonic oscillations. Suppose the sphere to be moving parallel to the axis of z , the velocity at any time t being represented by the real part of $\omega \epsilon^{ipt}$. Then if we take rectangular axes passing through the centre of the sphere and moving with it, the following equations are true inside the sphere if u, v, w are the components of the current, a, b, c those of magnetic induction, ψ the electrostatic potential, F ,

G, H the components of the vector potential, and σ the specific resistance of the metal.

$$\left. \begin{aligned} \sigma u &= -b\omega\epsilon^{ipt} - \frac{dF}{dt} - \frac{d\psi}{dx}, \\ \sigma v &= -a\omega\epsilon^{ipt} - \frac{dG}{dt} - \frac{d\psi}{dy}, \\ \sigma w &= -\frac{dH}{dt} - \frac{d\psi}{dz}; \end{aligned} \right\} \dots (2)$$

and therefore

$$\begin{aligned} -\frac{\sigma}{4\pi\mu'} \nabla^2 F &= -b\omega\epsilon^{ipt} - \frac{dF}{dt} - \frac{d\psi}{dx}, \\ -\frac{\sigma}{4\pi\mu'} \nabla^2 G &= a\omega\epsilon^{ip} - \frac{dG}{dt} - \frac{d\psi}{dy}, \\ -\frac{\sigma}{4\pi\mu'} \nabla^2 H &= -\frac{dH}{dt} - \frac{d\psi}{dz}. \end{aligned}$$

In the dielectric outside the sphere, if f, g, h are the electric displacements, K the specific inductive capacity, and if $\partial/\partial t$ denote partial differentiation with respect to the time, the equations are

$$\begin{aligned} \frac{4\pi}{K} f &= -\frac{dF}{dt} - \frac{d\psi}{dx} = -\left(\frac{\partial}{\partial t} - \omega\epsilon^{ipt} \frac{d}{dz}\right) F - \frac{d\psi}{dx}, \\ \frac{4\pi}{K} g &= -\frac{dG}{dt} - \frac{d\psi}{dy} = -\left(\frac{\partial}{\partial t} - \omega\epsilon^{ipt} \frac{d}{dz}\right) G - \frac{d\psi}{dy}, \\ \frac{4\pi}{K} h &= -\frac{dH}{dt} - \frac{d\psi}{dz} = -\left(\frac{\partial}{\partial t} - \omega\epsilon^{ipt} \frac{d}{dz}\right) H - \frac{d\psi}{dz}; \end{aligned}$$

and therefore

$$-\frac{1}{\mu K} \nabla^2 F = -\left(\frac{\partial}{\partial t} - \omega\epsilon^{ipt} \frac{d}{dz}\right)^2 F - \left(\frac{\partial}{\partial t} - \omega\epsilon^{ipt} \frac{d}{dz}\right) \frac{d\psi}{dz}. \quad (3)$$

with a similar equation for G.

From the form of these equations we see that the solution will take the form

$$\begin{aligned} \psi &= \psi_0 + \psi_1 \epsilon^{ipt} + \psi_1' \epsilon^{-ipt} + \psi_2 \epsilon^{2ipt} + \psi_2' \epsilon^{-2ipt} + \dots \\ F &= F_1 \epsilon^{ipt} + F_1' \epsilon^{-ipt} + F_2 \epsilon^{2ipt} + F_2' \epsilon^{-2ipt}. \end{aligned}$$

If we substitute these values in the above equations, we see that we may put $\psi_1', \psi_2', \dots, F_1', F_2', \dots$ all equal to zero.

If e is the quantity of electricity on the sphere,

$$\psi_0 = \frac{e}{K}.$$

Equating the coefficients of ϵ^{ipt} to zero in equations (2) and (3) we have

$$-\frac{\sigma}{4\pi\mu'}\nabla^2 F_1 = -\frac{dF_1}{dt} - \frac{d\psi_1}{dx} \text{ inside the sphere,}$$

with similar equations for G_1 and H_1 ; outside the sphere we have

$$-\frac{1}{\mu K}\nabla^2 F_1 = -\frac{\partial^2 F_1}{\partial t^2} - \frac{d^2 \psi_1}{dt dx} + \frac{\omega e}{K} \frac{d^2}{dx dz} \frac{1}{r}, \quad (4)$$

with similar equations for G_1 and H_1 .

The form of equation (4) suggests that we should put

$$\psi_1 = B \frac{d}{dz} \frac{1}{r}.$$

A particular integral of (4) is then

$$F_1 = \frac{cB}{p} \frac{d^2}{dx dz} \frac{1}{r} - \frac{\omega e}{K p^2 dx dz} \frac{1}{r}.$$

The complementary function is that solution of the differential equation

$$\nabla^2 F_1 + \mu K p^2 F_1 = 0,$$

which, when considered as a function of the angular coordinates of a point, varies as $r^3 \frac{d^2}{dx dz} \frac{1}{r}$; this (see Proc. Math. Soc. vol. xv. p. 212) is

$$C E_2(i\lambda r) r^3 \frac{d^2}{dx dz} \frac{1}{r},$$

where

$$E_2(i\lambda r) = \frac{3\epsilon^{-i\lambda r}}{(i\lambda r)^3} + \frac{3\epsilon^{-i\lambda r}}{(i\lambda r)^2} + \frac{\epsilon^{-i\lambda r}}{i\lambda r},$$

and

$$\lambda^2 = \mu K p^2.$$

Thus, outside the sphere,

$$F_1 = C E_2(i\lambda r) r^3 \frac{d^2}{dx dz} \frac{1}{r} + \frac{iB\epsilon^{pt}}{p} \frac{d^2}{dx dz} \frac{1}{r} - \frac{\omega e \epsilon^{pt}}{K p^2} \frac{d^2}{dx dz} \frac{1}{r},$$

$$G_1 = C E_2(i\lambda r) r^3 \frac{d^2}{dy dz} \frac{1}{r} + \frac{iB\epsilon^{pt}}{p} \frac{d^2}{dy dz} \frac{1}{r} - \frac{\omega e \epsilon^{pt}}{K p^2} \frac{d^2}{dy dz} \frac{1}{r},$$

$$H_1 = C E_2(i\lambda r) r^3 \frac{d^2}{dz^2} \frac{1}{r} - 2C E_0(i\lambda r) + \frac{iB}{p} \epsilon^{pt} \frac{d^2}{dz^2} \frac{1}{r} - \frac{\omega e \epsilon^{pt}}{K p^2} \frac{d^2}{dz^2} \frac{1}{r},$$

where $E_0(i\lambda r) = \epsilon^{-i\lambda r}/i\lambda r$, and is introduced into the expression

for H to make

$$\frac{dF}{dx} + \frac{dG}{dy} + \frac{dH}{dz} = 0.$$

Inside the sphere the differential equations for F_1 and G_1 are of the form

$$-\frac{\sigma}{4\pi\mu'} \nabla^2 F_1 = -ipF_1;$$

if $\lambda_1^2 = -4\pi\mu'ip/\sigma$, the solution of this equation is

$$F_1 = D\epsilon^{ipt} S_2(\lambda_1 r) r^3 \frac{d^2}{dx dz} \frac{1}{r},$$

where

$$S_2(\lambda_1 r) = \frac{3 \sin \lambda_1 r}{(\lambda_1 r)^3} - \frac{3 \cos \lambda_1 r}{(\lambda_1 r)^2} - \frac{\sin \lambda_1 r}{\lambda_1 r}.$$

Similarly

$$G_1 = D\epsilon^{ipt} S_2(\lambda_1 r) r^3 \frac{d^2}{dy dz} \frac{1}{r};$$

the differential equation for H_1 is

$$-\frac{\sigma}{4\mu'} \nabla^2 H_1 = -ipH_1 + \frac{B}{a^3}.$$

So that

$$H_1 = D\epsilon^{ipt} S_2(\lambda_1 r) r^3 \frac{d^2}{dz^2} \frac{1}{r} + 2D\epsilon^{ipt} S_0(\lambda_1 r) + \frac{B}{ip a^3} \epsilon^{ipt},$$

where

$$S_0(\lambda_1 r) = \frac{\sin \lambda_1 r}{\lambda_1 r},$$

and is introduced to make

$$\frac{dF}{dx} + \frac{dG}{dy} + \frac{dH}{dz} = 0.$$

Since F_1 , G_1 , H_1 are continuous when $r=a$, if a is the radius of the sphere we have

$$\left. \begin{aligned} CE_2(i\lambda a) - DS_2(\lambda_1 a) + \frac{iB}{pa^3} &= \frac{\omega e}{p^2 a^3 k}, \\ -2CE_0(i\lambda a) - 2DS_0(\lambda_1 a) + \frac{iB}{pa^3} &= 0. \end{aligned} \right\} \quad \dots (5)$$

Since, on the assumption discussed above, the electrification on the surface of the moving sphere is equivalent to a tangential current-sheet whose intensity is $\omega \epsilon^{ipt} \sin \theta \frac{e}{4\pi a^2}$, we have as another surface-condition that the difference between the

magnetic force outside and inside $= \omega \epsilon^{ipt} \sin \theta \frac{e}{a^2}$; hence

$$-3C \frac{d}{da} E_0(i\lambda a) - 3D \frac{dS_0(\lambda_1 a)}{da} = \frac{\omega e}{a^2}.$$

From (5) we have

$$C(E_2(i\lambda a) + 2E_0(i\lambda a)) - D(S_2(\lambda_1 a) - 2S_0(\lambda_1 a)) = \frac{\omega e}{p^2 a^3 K}.$$

From these equations we have

$$C = \frac{1}{\Delta} \left\{ \frac{\omega e}{K p^2 a^3} \frac{d}{da} S_0(\lambda_1 a) - \frac{1}{3} \frac{\omega e}{a^2} (S_2(\lambda_1 a) - 2S_0(\lambda_1 a)) \right\} \quad (6)$$

$$D = \frac{1}{\Delta} \left\{ -\frac{1}{3} \frac{\omega e}{a^2} (E_2(i\lambda a) + 2E_0(i\lambda a)) - \frac{\omega e}{K p^2 a^3} \frac{d}{da} E_0(i\lambda a) \right\}, \quad (7)$$

where

$$\Delta = \left(E_2(i\lambda a) + 2E_0(i\lambda a) \frac{d}{da} S_0(\lambda_1 a) + \frac{d}{da} E_0(i\lambda a) (S_2(\lambda_1 a) - 2S_0(\lambda_1 a)) \right)$$

Let us first consider the case where λa and λ_1 are both small. In this case $E_2(i\lambda a)$ is large compared with $E_0(i\lambda a)$ and $S_2(\lambda_1 a)$, very small compared with $S_0(\lambda_1 a)$; hence we see that

$$C = \frac{\omega e}{K p^2 a^3} E_2(i\lambda a),$$

$$D = - \frac{-\frac{\omega e}{a^3} E_0(i\lambda a)}{E_2(i\lambda a) \frac{d}{da} S_0(\lambda_1 a)}.$$

Since $E_2(i\lambda a) = \frac{3\epsilon^{-i\lambda a}}{(i\lambda a)^3}$ approximately, and $S_0(\lambda_1 a) = 1$, we have

$$C = -\frac{1}{3} i\omega e \lambda \epsilon^{i\lambda a},$$

$$D = \frac{\omega e}{a} \frac{ip\sigma K}{4\pi}.$$

The magnetic force outside the sphere parallel to the axis of x equals

$$\frac{dH_1}{dy} - \frac{dG_1}{dz},$$

or

$$\begin{aligned} & -3C \frac{d}{dy} E_0(i\lambda r) \epsilon^{ipt}, \\ & = i\omega e \lambda \epsilon^{i\lambda a} \frac{d}{dy} \frac{\epsilon^{-i\lambda r}}{i\lambda r} \cdot \epsilon^{ipt}, \end{aligned}$$

taking the real part

$$= \omega e \frac{d}{dy} \frac{\cos (pt - \lambda(r-a))}{r},$$

Similarly the magnetic force parallel to the axis of x

$$= -\omega e \frac{d}{dx} \frac{\cos (pt - \lambda(r-a))}{r},$$

and the magnetic force parallel to z vanishes. Thus the magnetic force is the same as that which would be produced by a current-element $\omega e \cos pt$ or ev , v being the velocity of the sphere (see Proc. Math. Soc. xv. p. 214).

The magnetic force inside the sphere parallel to x equals

$$\begin{aligned} & 3D\epsilon^{ipt} \frac{d}{dy} S_0(\lambda_1 r) \\ &= 3D\epsilon^{ipt} \frac{d}{dr} S_0(\lambda_1 r) \frac{y}{r}. \end{aligned}$$

Substituting the value for D given by equation (7), this equals

$$\frac{\omega e}{a} p^2 K y \epsilon^{ipt};$$

or, taking the real part and writing λ^2 for Kp^2 ,

$$\frac{\omega e}{a^3} (\lambda^2 a^2) y \cos pt.$$

The component parallel to y is

$$- \frac{\omega e}{a^3} (\lambda^2 a^2) x \cos pt,$$

and the z -component vanishes. Thus the maximum magnetic force inside the sphere is

$$\frac{\omega e}{a^2} \cos pt (\lambda a)^2.$$

If λa is very small, this is very small compared with the force outside the sphere. If the velocity is uniform, p , and therefore $\lambda=0$, and the magnetic force inside the sphere vanishes. When there is no magnetic force inside the sphere its energy and the force acting upon it have the values assigned to them by Mr. Heaviside.

Let us next take the case where λa is small and $\lambda_1 a$ large: in this case C and D have the same values as before, so that the magnetic force due to the moving sphere is the same.

We must now consider the case where λa and $\lambda_1 a$ are both large ; in this case we find from (6) and (7)

$$C = \frac{\omega e}{K p^2 a^3} \frac{i \lambda a}{3 \epsilon^{-i \lambda a}},$$

if $\lambda_1 a / \lambda a$ and $\lambda_1 a / \lambda^2 a^2$ are both large.

Thus the magnetic force parallel to x outside

$$= \frac{1}{3} \frac{\omega e}{\lambda^2 a^2} \frac{d}{dy} \frac{\cos \{pt - \lambda(r - a)\}}{r},$$

and that parallel to y

$$= - \frac{1}{3} \frac{\omega e}{\lambda^2 a^2} \frac{d}{dx} \frac{\cos \{pt - \lambda(r - a)\}}{r}.$$

Thus, since $\lambda^2 a^2$ is large, the magnetic force, though in the same direction as that due to a current ωr , is very much smaller in magnitude, and fades away to zero as λa increases without limit.

$$D = - \frac{\omega e}{3 a^2 \frac{d}{da} S_0(\lambda_1 a)}.$$

The maximum magnetic force inside the sphere

$$3D \frac{d}{da} S_0(\lambda_1 a) \epsilon^{ipt} = - \frac{\omega e}{a^2} \cos pt.$$

Thus in this case the magnetic force just inside the sphere is equal to $-ve$, while that outside the sphere is very much smaller. This is a striking contrast to the previous cases, where the magnetic force inside the sphere is very small compared with that outside. Thus, in this case, when the time of the oscillation is small compared with that of the electrical oscillations the distribution of magnetic force is turned inside out. The magnetic force diminishes very rapidly as we recede from the surface of the sphere. In this case the total current parallel to the axis of z inside the sphere is finite, for this by equation (2) equals

$$\begin{aligned} & - \frac{ip}{\sigma} 2D \int_0^a S_0(\lambda_1 r) 4\pi r^2 dr, \\ & = \frac{ip}{\sigma} \frac{2D 4\pi}{\lambda_1^2} \epsilon^{ipt} a^2 \frac{d}{da} S_0(\lambda_1 a) \\ & = - \frac{2}{3} e \omega \cos pt. \end{aligned}$$

So that if the sphere is placed in a magnetic field the force acting upon it is the same as that on a current $\frac{1}{2}e\omega \cos pt$.

When the sphere is moving with a uniform velocity ω , equations (3) become

$$-\frac{1}{\mu K} \nabla^2 F = -\omega^2 \frac{d^2 F}{dz^2} + \omega \frac{d^2 \Psi}{dx dz};$$

whence

$$\frac{d^2 a}{dx^2} + \frac{d^2 a}{dy^2} + \frac{d^2 a}{dz^2} \left(1 - \frac{\omega^2}{v^2}\right) = 0,$$

where v is the velocity of propagation of electrodynamic action through the dielectric. If we put

$$z = \left\{1 - \frac{\omega^2}{v^2}\right\}^{\frac{1}{2}} z',$$

this equation becomes

$$\frac{d^2 a}{dx^2} + \frac{d^2 a}{dy^2} + \frac{d^2 a}{dz_1^2} = 0.$$

With similar equations for b and c , we see that a solution of these equations is

$$\begin{aligned} a &= k \frac{d}{dy} \frac{1}{\sqrt{x^2 + y^2 + z_1^2}}; \\ b &= -k \frac{d}{dx} \frac{1}{\sqrt{x^2 + y^2 + z_1^2}}; \\ c &= 0; \end{aligned}$$

where k is a constant. Since, if $\mu = 1$,

$$4\pi \frac{df}{dt} = \frac{dc}{dy} - \frac{db}{dz},$$

and

$$\frac{df}{dt} = -\omega \frac{df}{dz},$$

we have

$$-4\pi\omega f = -b = k \frac{d}{dx} \frac{1}{\sqrt{x^2 + y^2 + z_1^2}}.$$

Similarly

$$-4\pi\omega g = +a = k \frac{d}{dy} \frac{1}{\sqrt{x^2 + y^2 + z_1^2}},$$

and

$$-4\pi\omega \frac{dh}{dz} = \frac{db}{dx} - \frac{da}{dy} = k \frac{d^2}{dz_1^2} \frac{1}{\sqrt{x^2 + y^2 + z_1^2}};$$

so that

$$-4\pi\omega h = k \sqrt{1 - \frac{\omega^2}{v^2} \frac{d}{dz_1} \frac{1}{\sqrt{x^2 + y^2 + z_1^2}}}.$$

The displacement across any spherical surface must $=e$, so that

$$\int (xf + yg + zh) dS = ae;$$

and therefore, if $u < v$,

$$\frac{k}{2\omega} \int_0^\pi \frac{\sin \theta d\theta}{\left(1 - \frac{u^2}{v^2} \sin^2 \theta\right)^{\frac{3}{2}}} = \frac{e}{\left(1 - \frac{u^2}{v^2}\right)^{\frac{3}{2}}};$$

$$k = \omega e \left(1 - \frac{u^2}{v^2}\right)^{-\frac{1}{2}}.$$

Thus the lines of magnetic forces are circles round the axis of z and the magnitude of the force equals

$$\frac{\omega e \sin \theta \left(1 - \frac{\omega^2}{v^2}\right)}{r^2 \left\{ \left(1 - \frac{\omega^2}{v^2} \sin^2 \theta\right) \right\}^{\frac{3}{2}}},$$

which is Mr. Heaviside's result. If $\omega > v$, the integral becomes infinite, the displacement will be within a cone of semi-vertical angle $\sin^{-1} \frac{v}{\omega} = \beta$; we must therefore only integrate within this cone, and the equation to determine k is

$$\frac{k}{2\omega} \int_\pi^{\sin^{-1} \frac{\omega}{v}} \frac{\sin \theta d\theta}{\left(1 - \frac{u^2}{v^2} \sin^2 \theta\right)^{\frac{3}{2}}} = \frac{e}{\left(1 - \frac{u^2}{v^2}\right)^{\frac{3}{2}}};$$

or

$$k = \frac{\omega e \left(1 - \frac{u^2}{v^2}\right)^{-\frac{1}{2}}}{\cos \beta} \left(1 - \frac{\omega^2}{v^2} \sin^2 \beta\right)^{\frac{1}{2}}.$$

Thus the magnetic force

$$= \frac{\omega e \left(1 - \frac{u^2}{v^2}\right)^{\frac{1}{2}} \left(1 - \frac{\omega^2}{v^2} \sin^2 \beta\right)^{\frac{1}{2}}}{\cos \beta r^2 \left(1 - \frac{\omega^2}{v^2} \sin^2 \theta\right)^{\frac{3}{2}}}.$$

Since $\sin \beta = v \omega$, this expression vanishes unless $\theta = \beta$, when it becomes infinite, so that the magnetic force and the electric displacement seem confined to the surface of a cone of semi-vertical angle β , the vertex pointing in the direction of motion.

Trinity College, Cambridge.
April 24, 1889.

II. *Quartz as an Insulator.* By C. V. BOYS, A.R.S.M., F.R.S.,
Assistant Professor of Physics at the Normal School of
Science, South Kensington*.

[Plate I.]

WHEN making quartz threads by the bow-and-arrow process described in the Philosophical Magazine, June 1887, I have sometimes noticed that the thread does not reach all the way from the arrow to the bow, but that the end remains suspended in mid air somewhere between. When this is the case the last foot about, then very fine, is usually in the form of an irregular helix. Under these circumstances, if the hand is brought at all near the end the helix stretches itself out, and the end of the thread flies to and attaches itself to the hand. On removing the hand the thread takes its old form; and this may be repeated several times.

It did not seem possible to account for this in any way except by supposing the thread to be electrified, though why it should be electrified is not clear. If this is the case then the insulating-power of the thread must be very great, for with the very small quantity of electricity which could remain on a body of such immeasurably small capacity, all trace of charge would escape instantly if the thread insulated no better than glass in the open air.

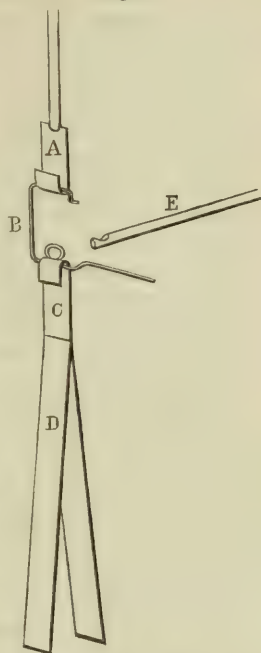
I therefore thought it would be interesting to see if rods of fused quartz showed any great superiority over similar rods of glass under the same circumstances. The plan that I have followed has been to hang a pair of very narrow gold leaves from the rod under examination, and observe the rate at which they closed after being charged. I purposely avoided all instruments the large capacity of which would increase the time of discharge, and the leakage of which might be comparable with or even exceed that of the rods to be tested.

The arrangement of leaves &c. is shown in fig. 1. A flat brass hook, A, is fastened to a rod which can slide stiffly through the

* Communicated by the Physical Society: read April 13, 1889.

centre of the lid of a mahogany box lined with tinfoil. From this is suspended the piece of glass or quartz, B, bent to the form shown, so that it can be handled by the projecting end without touching the portion that acts as the insulator. On this hangs a piece of bent brass, C, to which the leaves, D, are attached. A small ring of wire is soldered to the upper end of C, which is used when one insulator is changed for another as follows:—

Fig. 1.



A stiff wire, E, passes through one side of the instrument, and this is pushed forward through the wire ring. A is then depressed until the lower side of B hangs clear of the hook C. E is then drawn back with C and D suspended from it. Finally, when B is changed, E is pushed forward again, and A raised until the ring on C is just free from E, which is then withdrawn. By this means the leaves are always left at the same level. The leaves are 90×5 millim., and the box, which has a glass front and back, is 285 millim. high \times 130 millim. wide, and 185 millim. from front to back. The leaves are suspended so that the line of junction of C and D is 160 millim. above the base of the instrument. The length of the insulating portion of the quartz and glass hooks B is 21 millim. and the diameter about 1 millim. The leaves were observed by fixing the object-glass of a telescope at a distance of 393 millim., which projected an image of the leaves on a scale 1940 millim. beyond the lens. Thus the observed divergences were 4.93 times the true distances between the ends of the leaves.

The results of the experiments can be seen from fig. 2 (Plate I.). The rate at which the leaves close is the same with lead-glass in air dried by sulphuric acid, with quartz in air dried by sulphuric acid, and with quartz in air kept moist by means of a large flat dish of water. Soda-glass in air dried by sulphuric acid allowed the electricity to escape about eleven times as fast. With either kind of glass in moist air the charge escaped almost at once, but soda-glass was much worse than lead-glass. The glass had in all cases been boiled

in distilled water, a process which Warburg and Ihmori* have shown is necessary in order to make the glass insulate as well as possible. There is no appreciable difference between the rate at which positive and negative electricity escapes from the leaves.

The quartz insulator was then treated in various ways to see how well it is likely to retain its insulating power. It was boiled for five minutes in a weak solution of potash and washed. It was boiled for the same time in a strong solution of potash and washed. In both cases it insulated as before. It was dipped for two minutes in melted potash and washed. In moist air it insulated better than either soda or lead-glass, but not so well as before treatment with potash. Boiling in strong hydrochloric acid did not restore the lost power. A new hook was not affected by boiling in strong hydrochloric acid, or by heating in a batswing gas-flame.

Perhaps the most surprising result is obtained by dipping the quartz hook in water or ammonia, and immediately hanging on the leaves while the water is standing upon the hook in beads. Even so no difference is observed in its insulating-power. If it is dipped in a solution of potash this is not the case; but of course the insulation is restored by washing.

The perfect equality of the rate at which the charge escapes when the leaves are suspended from lead-glass in dry air, or quartz in dry or moist air, makes it probable that this loss of charge is not due to leakage along the insulator, for it is very unlikely that, under these different circumstances, the loss should be exactly alike. It is more probable that the loss is due mainly to convection through the air. This is made certain by the following considerations. The same leaves, when hung by the same hook in another box which was badly made and rough inside, lost their charge much more quickly, but, as before, at the same rate in the three cases. On the other hand the leaves, when suspended in the best instrument by a quartz fibre about ten times as long and one hundredth of the diameter of the piece B (that is, by one which would insulate a thousand times as well if the loss was due to surface creeping, or a hundred thousand times as well if it were due to actual conduction), lost their charge practically at the same rate as before.

Pieces of polished rock-crystal, such as are used as objects for the polariscope, also insulate well; but they do not seem to be quite so free from the influence of moisture as the fused quartz. The same is true with regard to the natural faces and the fractured surface of the crystal.

The electromotive forces required to produce different diver-

* Wied. *Ann.* xxvii. p. 481 (1886).

gences of the gold leaves were determined by an absolute electrometer, and the results are shown in fig. 1 (Plate I.).

It is probable that this valuable property of quartz, that it insulates perfectly in damp air, may be of use in the construction of electrostatic apparatus. The sulphuric acid now absolutely necessary in electrometers and instruments of that class is nothing short of a nuisance. If the instrument is carried about there is the risk of destruction of the instrument from the spilling of the acid. If the instrument is not moved the acid, unless specially treated, may give off nitrous fumes which will corrode the surfaces of metal; or, if forgotten, it absorbs water and in time overflows, destroying the whole apparatus. Even if the air were saturated with moisture, rods of quartz would insulate as well as the lead-glass at present used does in air dried by sulphuric acid. The needle should of course be suspended by a fibre of quartz, which is far simpler to apply and adjust than the double line of silk, and superior also in other respects.

In conclusion I must express my obligations to Mr. Briscoe, a student in the laboratory, whose skill in the manipulation of gold leaf and whose suggestions from time to time have been of the greatest service. I have with perfect confidence asked him to carry out the experiments described in this paper, and the results show that the confidence was not misplaced.

III. *A Comparison of the Mercury Unit with the British Association Unit of Resistance.* By CARY T. HUTCHINSON and GILBERT WILKES*.

THE object of this research, which was conducted in the Physical Laboratory of the Johns Hopkins University, under the supervision of Professor Henry A. Rowland, is the determination of the ratio of the resistance, at 0° C., of a column of mercury, 1 metre long and 1 square millimetre in cross section, to the British Association unit of electrical resistance.

The method employed in making the observations was, with slight modifications, the same as that used by Lord Rayleigh, by Glazebrook and Fitzpatrick, and also in a similar determination already made at this laboratory.

The resistance at 0° C. of a column of mercury, filling a fine, accurately calibrated glass tube, is determined in British Association units; the length L is known; its mean cross

* From the 'Johns Hopkins University Circular' for May 1889. Communicated by the Authors.

section at 0° C. is found by weighing the volume of mercury, contained at that temperature, and dividing this by L multiplied by the density of mercury (ρ) in grammes per cubic centimetre.

The resistance of a column of mercury of varying cross section is calculated as follows (Maxwell's 'Electricity and Magnetism') :—

Let s be the cross section of the tube at a distance x from one end ; let λ be the length of a short thread of mercury, when its middle point is distant x from this end ; then, assuming s constant, throughout the length λ , we have $s = \frac{C}{\lambda}$, where C is the constant volume of the thread.

The weight of mercury that fills the tube is

$$W = \rho \int s dx = \rho C \Sigma \left(\frac{1}{\lambda} \right) \frac{L}{n}, \quad . \quad . \quad . \quad . \quad (1)$$

in which n is the number of points, at equal distances along the tube, where λ is measured.

The resistance of the tube full of mercury is

$$R = \int \frac{r'}{s} dx = \frac{r'}{C} \Sigma(\lambda) \frac{L}{n}, \quad . \quad . \quad . \quad . \quad (2)$$

where r' is the specific resistance of mercury for unit volume. Hence, from (1) and (2),

$$WR = r' \rho \Sigma(\lambda) \Sigma \left(\frac{1}{\lambda} \right) \frac{L^2}{n^2},$$

or

$$r = \frac{10^4 WR n^2}{\rho L^2 \Sigma(\lambda) \Sigma \left(\frac{1}{\lambda} \right)},$$

in which r is the resistance of a column of mercury 1 metre long and 1 square millimetre in cross section, at 0° C., expressed in British Association units.

In this equation put

$$\frac{\Sigma(\lambda) \Sigma \left(\frac{1}{\lambda} \right)}{n^2} = \mu.$$

The equation for r now becomes

$$r = 10^4 \frac{RW}{\rho \mu L^2} \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

R, W, r , ρ , L have already been defined. L is measured in centimetres ;

μ is the coefficient correcting for conicality of the tube.

Let

L = length of tube at t'° , measured by brass bar at $t_b'^{\circ}$;

l = " thread of mercury filling the tube at 0° , referred to bar at $t_b'^{\circ}$;

δL = correction to L, for junction of column of mercury with terminals = $\cdot 82$ diameter of tube ;

ρ = specific gravity of mercury at 0° C. = $13\cdot 595$;

y = cubic expansion of mercury per degree = $\cdot 0001795$;

g = " " glass " " = $\cdot 000025$;

b = linear " bar " " = $\cdot 000019$;

t_0 = temperature of brass bar, to which lengths are reduced, = $8^{\circ}\cdot 7$ C.

Then, volume of thread at 0° C. = $\frac{W}{\rho}$.

Mean section of tube at 0°

$$= \frac{W}{\rho} \frac{1}{l\{1+b(t_b'-t_0)\}}$$

$$= \frac{W}{\rho} \cdot \frac{(1+\frac{1}{3}gt')}{L\{1+b(t_b'-t_0)\}}.$$

Reduced length of tube at 0°

$$= \frac{(L+\delta L)\{1+b(t_b'-t_0)\}}{1+\frac{1}{3}gt'}.$$

Hence

$$R = \frac{10^{-4}r \cdot \mu \cdot (L+\delta L)\{1+b(t_b'-t_0)\}}{1+\frac{1}{3}gt'} \cdot \frac{\rho L\{1+b(t_b'-t_0)\}}{W(1+\frac{1}{3}gt')}.$$

Solving for r ,

$$r = \frac{10^4 RW(1+\frac{1}{3}gt')^2}{\rho \mu L(L+\delta L)\{1+b(t_b'-t_0)\}\{1+b(t_b'-t_0)\}}$$

$$= \frac{10^4 RW(1+\frac{1}{3}gt')^2}{\rho \mu L^2} \left(1 - \frac{\delta L}{L}\right) \{1 - 2b(t_b'-t_0)\},$$

or

$$r = \frac{10^4 RW}{\rho \mu L^2} \left(1 - \frac{\delta L}{L}\right) (1 + \frac{2}{3}gt') \{1 - 2b(t_b'-t_0)\}. \quad (4)$$

The ends of the tube containing the mercury opened into the ebonite cups about two thirds filled with mercury. Upon the assumption that these may be considered infinitely large

in comparison with the diameter of the tube, Lord Rayleigh has calculated that a correction of $\cdot 82$ diameter, additive to the true length of the tube, is necessary in order to allow for the resistance of the terminal connexions. Mascart, Nerville and Benoit, and also Glazebrook and Fitzpatrick have verified this result experimentally. This quantity is δL in the above formulæ.

In commencing our experiments in the spring of 1888, our first object was to determine the best methods that had been used for the different determinations involved. About two months were spent in standardizing resistances that were to be used (the comparisons being made in a constant temperature vault, using a Fleming bridge) and in testing different methods of measurement.

An attempt to measure the lengths at 0° C. was made, only the portion of the tube to be observed by the microscopes being scraped clear of ice. It was found that this method presented great difficulties; and as an error of 10 per cent. in the assumed expansion of glass, in reducing the length of the tube from 20° C. to 0° C., would only cause an error of $\frac{16}{1000000}$ in the length, the plan was abandoned.

The measurement of a column of mercury a little less than the length of the tube, which was covered with ice except at points over the ends of the column, was tried repeatedly, both by observing the meniscus and by flattening the ends of the mercury with hard rubber plugs (as suggested by Lord Rayleigh) without success. The lenses of the microscopes would naturally coat rapidly with moisture, and the unoccupied parts of the bore of the tube become so wet that minute globules of mercury would be left behind when the column was run out to be weighed. We endeavoured to obviate this by plugging the ends with soft wooden plugs; but still the moisture got in, making the meniscus uncertain, and interfering with the removal of the column.

Plate-glass end-pieces, held in place by elastic bands, were tried, hoping thus to obtain full tubes at 0° ; but, owing to the grinding of the plates against the mouths of the tubes and the old trouble with wet mercury, these were given up.

The tubes were at first secured to straight, narrow, well-seasoned boards, and (the end-cups being in place) were put in watertight rectangular boxes (lined with waxed duck) about 5 inches wide and 5 inches deep. Crushed ice was then packed in over them. Though observations taken ten or fifteen minutes apart would apparently agree, it was found that, owing to the proximity of the board, the mercury would sometimes not have reached its minimum resistance in four

or four and a half hours. The boards were therefore replaced by narrow partition-blocks, scored to allow the tubes to rest firmly. The tubes were thus raised about three quarters of an inch from the bottom of the box.

Most observers have measured the length of the column of mercury, used in determining the cross section of a tube, at the temperature of the room (between 10° and 20° C.), and then used a formula which reduces their observations to 0° C. As the cubical coefficient of expansion of mercury in glass is $\cdot 00016$, an error of little more than six tenths of a degree will make a difference of one part in ten thousand in the final result. Since the mercury-column is in a thick-walled glass tube, simply exposed to the air of the room (generally for a few hours), the uncertainty of its being at the temperature shown by thermometers placed alongside the tube may be readily seen. The tendency of this error will be to give too high a value for r . Glazebrook and Fitzpatrick measured the length of the column at intervals of fifteen minutes; and when two consecutive readings coincided, it was assumed that the mercury was at the temperature shown by the thermometers. They verified the result in several cases after the mercury had been blown out into a small capsule, but do not mention how they measured accurately the temperature of so small a volume of mercury.

In view of the results of our preliminary observations it was decided to determine the mean cross sections at zero, by using the mercury upon which the resistance-measurements had been made and obtaining a full tube as follows:—

When through with the resistance-measurements, one end-piece was removed and the tube stopped by one finger, over which was a tight, elastic, pure gum-band. The other end of the trough was then raised to an angle of about 20° without disturbing the tube in the ice, the end-piece quickly slipped off, the end of the mercury-column flattened off with a similarly covered finger, and any globules wiped away. The angle of the box being reversed, the mercury was allowed to flow out into a watch-glass, being afterwards dried over pumice-stone soaked with strong sulphuric acid.

Supposing that the exposed ends (about 6 centim. in all) rose to an average of 3° C., which they could hardly do in the few minutes necessary to empty the tube, as they were in such close proximity to the ice, and the original temperature less than $0^{\circ}\cdot 3$ C., the error due to this cause would make the result three parts in one hundred thousand too low.

Determination of μ .—The tubes were furnished by Eimer and Amend, of New York, and out of a very large number

about fifteen were selected on account of uniformity of bore, being tested by moving a small mercury column along in them and measuring its length with a scale. These were then more carefully tested and the best seven selected, which were cut as nearly as possible to represent exact multiples of a B.A. unit in resistance—one tube was cut for one half B.A. unit. The ends were ground convex, using a fine file and camphor in turpentine. After this the tubes were carefully cleaned, using distilled water, nitric acid, distilled water, ammonia, distilled water, alcohol, distilled water. Before using these liquids, a small piece of wet cotton-wool was drawn through the tubes (always in the same direction) in order to remove any solid particles that might accidentally be present. This was accomplished by first drawing through a silk thread by means of an air-pump, and then tying on the cotton and pulling it through several times in the same direction. This was always done in cleaning the tubes before filling. The tubes were dried by warm dry air, which had passed through calcium chloride and cotton-wool, the flow being kept up by a pneumatic pump.

The values of μ for the different tubes were obtained by two independent determinations, using different lengths of the thread of mercury. The lengths of the thread were read on a dividing-engine.

TABLE I.—Values of μ .

Tube.	Approximate resistance.	μ .	Length of thread, in centimetres.	μ .	Length of thread, in centimetres.	Mean μ .
I.	$\frac{1}{2}$ ohm.	1·00056	5	1·00050	3·9	1·00053
II.	$\frac{2}{2}$ „	1·00039	3	1·00043	3·8	1·00041
III.	2 „	1·00088	4·8	1·00080	4·0	1·00089
				1·00100	3·5	
IV.	10 „	1·00132	3·8	1·00133	4·8	1·00133
V.	5 „	1·00133	4·6	1·00122	4·0	1·00127
VI.	1 „	1·00055	4·7	1·00063	3·4	1·00060
				1·00062	3·8	

Determination of L.—As all the tubes except one were longer than a metre we calibrated two metres of a five-metre bar; but it was found so unwieldy that better results could be expected from using a metre bar and three microscopes. Accordingly a comparator was placed on a long marble slab, and in prolongation with it a third microscope, mounted on a solid wooden block cemented to the slab, was placed. The values of the micrometer-divisions were respectively ·0025 millim., ·0022 millim., ·0045 millim.

Ebonite plugs were inserted in the ends, and readings were taken in at least four positions, by revolving the tube, in each measurement.

The temperatures of the tube under observation and bar were given by two thermometers lying against each. Measurements were made by both observers.

TABLE II.—Lengths of Tubes, in terms of brass Metre (by Bartels and Diederichs, Göttingen).

Tube.	Length, in centimetres.	Length, in centimetres.	Average.	Temp. of tube.	Temp. of bar.
I.	127.7598	127.7610	127.761	19° C.	19.5° C
II.	129.8690	129.8726	129.871	18	18
III.	Broken in	preliminary	work.		
IV.	91.4500	91.4550	91.453	19	18.5
V.	181.0100	181.0147	181.012	19	20
VI.*	151.0951	151.1235	151.109	19.3	18
	151.1049	151.1034	151.104	24	24

The former value applies to determinations 1 and 2 of this tube, the latter to Nos. 3 and 4.

The temperature was very constant for the separate measurements on each tube and, as it never differed more than two degrees between corresponding observations, the average length is taken at the average temperature.

By comparison with the steel Rogers's standard, whose length is accurately known, the brass metre bar was found to equal 100.031 centim. at 25° C.; a result which agrees perfectly with previous determinations, using .000019 as the coefficient of linear expansion. This would make the bar correct at 8° 7 C.

Weighing.—Schickert weights and balances were used, the former being compared with the glass standard kilogramme, which has been compared with the Berlin standard.

In order to avoid errors due to moisture and uncertainty as to temperature, the standard and weights compared were left standing on the scale-pans for several hours before taking the weighings, which were made without opening the case of the balances. The temperature was kept constant, barometer readings taken, and the air was kept dry by calcium chloride. The brass kilogramme (K) was found to equal 1000.001 grammes *in vacuo*, which agrees well with former determinations. Specific gravity 8.3.

* One end became nicked and was smoothed off.

Resistances.—The resistances used were :—

1. Warden-Muirhead 10 B.A.U., No. 292. Value, determined by Glazebrook, October 1887, 9·99416 at 16°·5 C. Temperature-coefficient ·00292. This coil was our standard.

2. Elliott 10 B.A.U., which has been several times compared at the Cavendish Laboratory. It is marked as found correct by Rayleigh at 20°·9. By comparison with W.M. No. 292, we found it correct at 20°·8. Temperature-coefficient ·0034.

3. Elliott 1 B.A.U. Resistance ·99950 at 16° C. Temperature-coefficient ·00037.

4. Pratt 1 B.A.U. Resistance 1·02579 at 16° C. Temperature-coefficient ·00030.

5. A circular comparator (designed by Professor Rowland), containing ten 10 B.A.U. coils wound together on a copper cylinder, which contained water. The coils are protected by an outer cylinder which leaves a large air-space. Contacts are made by means of mercury cups, arranged circularly in an ebonite top. This comparator was used in standardizing the one-ohm coils and as a shunt. The coils were always kept standing in water for several hours before being used, and the temperature of this water was kept perfectly constant throughout the day by having in each vessel a coiled lead pipe, which was connected by rubber tubing with the pipes of the city water-supply.

All resistances were compared both before starting and after completing this portion of the work, and the two sets of results agreed.

The resistance of the rods used to connect the end terminals to the bridge was calculated for temperature of room, 18° C., ·001257. By observation :—temperature of the room 21°·5, ·001258. As this temperature never varied much from 22°, ·00127 was used as the correction in all cases.

A Fleming bridge was used, and the value of a division of the bridge-wire by two distinct determinations gave agreeing results. The resistances were so combined (except in the case of the first measurements made) as to require the use of as small a portion of the wire as possible.

Tube I. was balanced against coils E 1 B.A.U. and P 1 B.A.U. in parallel, shunted by coils 1, 2, 3, 5 of comparator in series. In 1st observation, shunt = coils 1 + 2.

Tube II., against E 1 B.A.U. + P 1 B.A.U. in series, shunted by coils 2, 3, 4, 5, 6 in series. In 1st observation, shunt = coil 1.

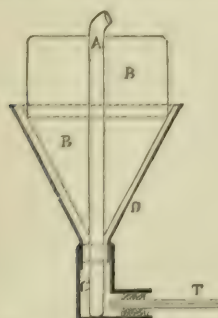
Tube III., broken in preliminary work.

Tube IV., against W.M. 10 B.A.U.

Tube V., against W.M. 10 B.A.U. and E 10 B.A.U. in parallel.
Tube VI., against E 1 B.A.U.

When coils were connected in parallel, their terminals rested solidly on copper disks, about $\frac{3}{4}$ inch in diameter, well amalgamated and covered with mercury, in boxwood cups. The rods connecting these cups to the bridge were short, stout copper rods, whose calculated resistance was 0.00156 at 16° C. This is, of course, always taken into account.

After cleaning the tube as described above, each end was thrust through a perforated cork, which was then fitted into an end-piece similar to those used by Lord Rayleigh, and the outer surfaces of the corks were covered with melted paraffin. The mercury used was new and was distilled in a vacuum, the temperature of distillation being low. Before filling the tube, the mercury was gently warmed and was then poured into one of the end-pieces, the other end being raised, in order to allow the column to flow in slowly. If any specks or small bubbles were noticed on the sides of the bore of the tube, the filling was discontinued and the tube again cleaned. The terminal cups were about two thirds full of mercury. These were corked, the tube laid in the notched



Scale $\frac{1}{4}$.

- A. Copper connecting Rod.
- B. Ice cup.
- C. Hard rubber terminal.
- D. Hard rubber.
- T. Tube.

partitions in the trough and well covered and surrounded with crushed ice. About four hours afterwards, the cups on the connecting rods (see figure) having been previously filled with ice, in order to have them cooled down before placing them in position, the resistance measurements were commenced and usually occupied about twenty minutes.

The galvanometer used was an Elliott, having a resistance of $1\frac{1}{2}$ ohms. A difference on the bridge-wire, amounting to one part in 100,000 of the resistance, being measured gave readable deflexions on either side of the "balance" position.

Three complete sets of observations were always taken

TABLE III.

	Tube.	No.	Date.	R.	L.	t' .	t'_0 .	$1 - \frac{\delta L}{L}$.	μ .	W.	$1 + \frac{2}{3}gt'$.	$1 - 2b(t'_0 - t'_0)$.	r .	Mean r .
1	I.	1		500587	127.761	19	19.5	.99887	1.00053	42.4108	1.00032	.99939	.952463	.95255
2		2		522						46.94			247	
3		3		424						42.10			255	
4	II.	1		1.63140	129.871	18	18	.99933	1.00041	13.4253	1.00030	.99965	.95340	.95320
5		2		110						42.74			337	
6		3		061						42.85			317	
7		4		075						42.73			317	
8		5		090						42.64			318	
9		6		066						42.76			313	
10		7		099						42.10			295	
11	IV.	1		9.97670	91.453	19	18.5	.99970	1.00133	1.08747	1.00032	.99963	.95246	.95343
12		2		867						88.45			331	
13		3		825						88.23			328	
14		4		8110						88.19			333	
15	V.	1		4.91311	181.012	19	20	.99970	1.00127	8.66358	1.00032	.99957	.95338	.95349
16		2		329						6.105			349	
17		3		405						6.062			357	
18		4		342						62.35			364	
19		5		413						58.48			335	
20	VI.	1		1.00545	151.109	19	18	.99924	1.00060	29.5039	1.00032	.99965	.95306	.95317
21		2		547						50.94			326	
22		3		553	151.104	24	24			50.81	1.00040	.99942	321	
23		4		553						50.58			314	

and the current reversed, in order to eliminate any small thermal current.

Benoit has determined that the resistance of mercury is decreased by the diffusion of copper amalgam from the ends of copper connecting rods. Similar experiments, performed at this laboratory, have demonstrated the fact that, by leaving the rods dipping in the mercury in the end-pieces for twenty-four hours, the resistance was decreased one part in twenty-four hundred. Therefore, the decrease due to this cause in our investigations must be inappreciable.

In the cases of nine fillings, a small thermometer was placed in the mercury cups immediately after removing the rods. In no case was the temperature thus obtained greater than $1^{\circ}\text{C}.$, and the average of the nine sets of observations was $0^{\circ}\cdot5\text{C}.$ Supposing that 6 centim. of the tube were at this temperature, in the worst case the error would be $\cdot00003$ high; while, in the other cases, it would be about $\cdot00002$ high.

Table III. (p. 26) gives our final results.

Owing to the tubes being entirely unprotected, great care was necessary in handling them. Tube III. was broken in our preliminary work, in trying to remove the end-pieces, which were then fitted with perforated rubber stoppers. These were after this replaced by common corks.

Tube I., whose bore was 1.76 millim. in diameter, allowed the soft rubber thimbles to sink into it and thus cause the column of mercury weighed to be smaller than it should have been. A correction should be applied on this account and might have been obtained by jacketing the tube with a water-jacket and then taking a number of alternating observations, using the thimbles and then glass plates. Unfortunately, this tube was broken after all the other observations had been completed. Its average—manifestly low—is not included in the final result.

Only one observation is neglected in the remaining tubes—II., IV., V., VI. An error of one part in a thousand is apparent in observation No. 1 of tube IV., which is the case referred to.

In the final result there are, therefore, on

Tube II., 7 observations.

„ IV., 3 „

„ V., 5 „

„ VI., 4 „

—

Total . . 19

Giving the tubes equal weights in the final average, we have :—

Tube II.	$r = \cdot 95319$
„ IV.	$r = \cdot 95343$
„ V.	$r = \cdot 95349$
„ VI.	$r = \cdot 95320$

Mean . . . $\cdot 95333$

Arranging the tubes in the order of ratio of length to diameter, we have

TABLE IV.

Tube.	Diameter.	Ratio of length to diameter.	r .
I.	millim. 1·763	724	$\cdot 95255$
VI.	1·352	1120	$\cdot 95317$
II.	·985	1320	$\cdot 95320$
V.	·670	2650	$\cdot 95349$
IV.	·334	2730	$\cdot 95343$

This table shows the effect of the sinking-in of the fingers, in taking the tubes full of mercury, for if we take tubes IV. and V., in which this ratio is roughly the same, the variations in r may be due to other causes. So too, tubes II. and VI., in which this ratio is roughly the same, agree.

Observations were taken, as suggested above, to determine the correction for tube VI. The quantity obtained is a difference and, therefore, difficult to determine accurately. The results are given below :—

TABLE V.—Correction necessary in case of Tube VI., to reduce for sinking-in of fingers in taking full tube.

Number of observation.	Correction in $\frac{1}{1000}$ of 1 per cent.
1	+ 44
2	+ 23
3	+ 15
4	+ 28
Average	$= + 27\cdot 5 = + \cdot 00026$

Average obtained from Tube VI. = .95317
 Correction = .00026

 Corrected result from Tube VI. = .95343

Taking the mean of tubes IV. and V., the variations of the other tubes are very nearly inversely proportional to the squares of these ratios.

Combining the results of tubes II., IV., V., VI., in this way, the final result would be .95341.

Combining the tubes with weights proportional to their resistances, that is proportional to $\frac{l}{d^2}$, the final result would be .95341.

Applying corrections, as given by Table V., we would have .95346.

We therefore give, as our value of the resistance of a column of mercury one metre long, one square millimetre in cross section, at 0° C.,

.95341.

TABLE VI.—Results obtained by different Experimenters.

Observers.	Date.	References.	Value of 1 metre of Mercury in B.A.U.
Lord Rayleigh & Mrs. Sidgwick	1883	Phil. Trans. 1883.	.95412
Mascart, Nerville & Benoit	1884	<i>Journal de Physique</i> , 1884.	.95374
Strecker	1885	{ Wiedemann's <i>Annalen</i> , vol. xxv. 1885.	.95334
L. Lorentz	1885	Same.	.95388
Rowland	1887	{ Communicated to British Association 1887. <i>Abhandl. der K. Bayer.</i> <i>Akad. der Wissenschaften</i> , II. Classe, vol. xvi. Abth. III.	.95349
Kohlrausch	1887	{	.95331
Glazebrook & Fitzpatrick...	1888	Phil. Trans. 1888.	.95352
Hutchinson & Wilkes	1888	.	.95341

IV. *Electrolytic Dissociation versus Hydration.*

By SVANTE ARRHENIUS*.

THE distinguished Russian chemist Mendeleeff has lately, in the Journal of the Russian Physico-chemical Society, dealt in an adverse manner with the theory of electrolytic dissociation. In that paper he expresses an opinion that his assumption of the existence of hydrates in solutions can be used to explain all the facts which hitherto have served as the foundation of the electrolytic-dissociation theory. As many English chemists (Armstrong, Crompton, Pickering) have in recent publications accepted and defended Mendeleeff's views, I take this opportunity of offering a few observations in an English scientific journal on Mendeleeff's paper.

In his paper the Russian savant makes use only of the data contained in the first memoir of van't Hoff (at the time of publishing which the latter was not acquainted with the theory of dissociation), and pays no attention to the later developments of the subject. Only in this way can Prof. Mendeleeff's concluding advice be explained, that before going further we must investigate whether the isotonic coefficients (i) of van't Hoff and de Vries are whole numbers or vary with the temperature and concentration. I have proved more than a year ago (*Zeitschr. für physikal. Chemie*, ii. p. 491) that the latter alternative is correct; *e. g.* for oxalic acid the values of i corresponding to the concentrations $\cdot 06$ and $\cdot 66$ gram-molecules per litre are $1\cdot 62$ and $1\cdot 37$ respectively. This can also be deduced from the electric conductivities of the solutions. Moreover I know of no one who has refused to accept this alternative. I refer, for example, to the more recent papers in the *Zeitschr. für physikal. Chemie* of de Vries and van't Hoff, both of whom at first held the other view. The statement that the isotonic coefficient of MgSO_4 at all concentrations is unity must also be corrected in the same sense; as I have found that this coefficient has the values $1\cdot 37$, $1\cdot 22$, and $1\cdot 04$ for the concentrations $\cdot 06$, $\cdot 16$, and $\cdot 66$ gram-molecules per litre. Thus the question, "how is it that in this case i for an electrolyte is unity?" is answered. Evidently by this neglect of a great part of what has been accomplished by the theory of dissociation Prof. Mendeleeff has come to the belief that the whole matter may easily be explained in another way. I therefore give an enumeration of the principal branches of physical science which have received an explanation from the hypotheses of osmotic pressure and of electrolytic dissociation.

* Communicated by the Author.

GROUP A.—(1) Osmotic pressure. (2) Lowering of freezing-point. (3) Lowering of vapour-pressure. (4) Raising of boiling-point. (5) Electromotive force of concentration-currents in solutions.

GROUP B.—(6) Electric conductivity of electrolytes.

GROUP C.—(7) Diffusion of electrolytic solutions.

GROUP D.—(8) Change of the degree of dissociation of weak acids with dilution. (9) Conductivity of mixed solutions. (10) Change of the strength of weak bases and acids by the addition of neutral salts. (11) Distribution of bases amongst different acids (Thomsen's "avidity").

GROUP E.—(12) Velocity of reactions of various chemical processes caused by the presence of acids or bases.

GROUP F.—Additive properties of electrolytic solutions, such as (13) specific volume and specific gravity. (14) Heat of neutralization. (15) Compressibility. (16) Internal friction. (17) Colour, rotatory power, and index of refraction.

It is by means of the two hypotheses named above that for the first time it has been made possible to calculate the numerical values of several thousand observations in these seventeen widely different fields; and with such success that no considerable contradiction between theory and experiment has arisen. Are we to assume that the view that hydrates exist in solutions can render such service? So far as I am aware not a single numerical datum has hitherto been deduced from this hypothesis.

I may be permitted to discuss this last question in a few words. In the first place it may be considered indubitable that it is impossible to determine whether a salt occurs in solution as hydrate or not by any of the methods for determining the properties enumerated in group A (except perhaps No. 5). Here we perfectly agree with Prof. Mendelejeff; his remarks besides are to be found almost word for word in a memoir of Raoult (*Ann. de Chim. et Phys.* [6] viii. p. 291). From this it follows that the conclusions, drawn by Rüdorff and Wüllner, from the lowering of the freezing-point and vapour-pressure of solutions, in favour of the existence of hydrates in them, are unfounded, as Tammann and others have already shown. These inadmissible conclusions of Rüdorff and Wüllner were formerly, however, considered as the chief proof of the existence of hydrates in salt solutions.

On the other hand, there are other phenomena belonging to the groups B, C, and F which are opposed to this assumption. If, for instance, an electric current is passed through a solution of KCl, of which we assume that it exists as the hydrate $\text{KCl} \cdot m\text{H}_2\text{O}$, then the ions of this salt are $\text{K} \cdot n\text{H}_2\text{O}$, and

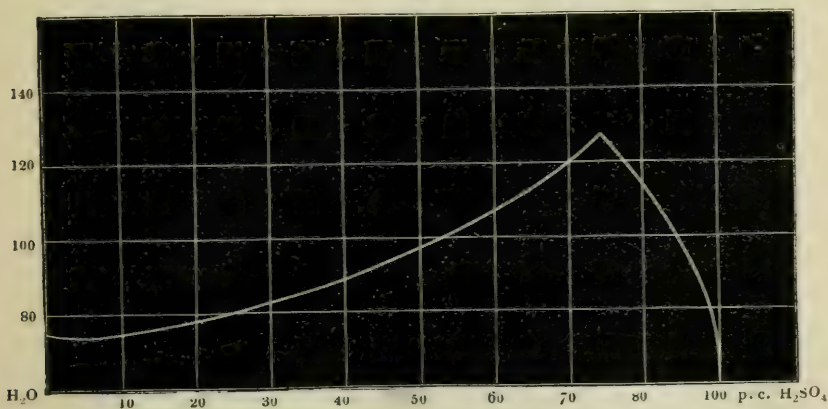
$\text{Cl}(m-n)\text{H}_2\text{O}$ (G. Wiedemann's hypothesis). Now it appears from Ostwald's researches that the velocity of an ion is the smaller the more atoms it contains. Thus the velocity (which may be easily determined from the conductivity) for the potassium ion of a solution of KCl , viz. $\text{K} \cdot n\text{H}_2\text{O}$, must be smaller the greater is the value of n . But Kohlrausch has shown that the ion $\text{K} \cdot n\text{H}_2\text{O}$ travels at the same rate in solutions of all the potassium salts, therefore the ion K must be combined with the same quantity of water in all salts. This holds for all other ions. As soon therefore as both the ions of a salt solution are given, then the hydrate is also known; and the composition of this hydrate does not alter with the concentration, which certainly does not agree with Mendelejeff's views. There are, besides, other circumstances (Ostwald, *Zeitschr. für physikal. Chemie*, ii. p. 840) which make it very probable that in the ions $\text{I} \cdot n\text{H}_2\text{O}$, n is extremely small; and this, again, according to Mendelejeff is not the case in dilute solutions. But as we have no ground for attributing any particular value to n , and as it is besides probable that many salts (*e. g.* most of those of potassium) exist only in the anhydrous state, the simplest and likeliest assumption is that the ions of the salts, and consequently the salts themselves, exist in solution without water of hydration. In an analogous way we come to precisely the same conclusion from considering diffusion and the additive properties of salt solutions. The theory of dissociation (contrary to Mendelejeff's assertion) is therefore decidedly unfavourable to the assumption of the existence in dilute solutions of hydrates with large quantities of water.

It is well known that for a very long time chemists have been striving to find hydrates in solutions from a consideration of the properties of the solutions. Graham some forty years ago lent his support to such a view. The mode of procedure was very simple. Any property, *e. g.* internal friction (Graham), was taken and tabulated as ordinates against the percentage of substance in solution as abscissæ. In the curves thus obtained are some singular points, *e. g.* maxima, minima, points of inflexion, angular points. In this way Graham found that the internal friction of solutions of alcohol in water had a maximum near 36 per cent. alcohol, and concluded therefrom that possibly this composition corresponded to a definite and highly viscous hydrate (perhaps $\text{C}_2\text{H}_5\text{OH} \cdot 5\text{H}_2\text{O}$). This conclusion is evidently devoid of any theoretical foundation, and is in fact simply a random shot. Consequently when it was found that the maximum varies with the temperature, this attempt at ex-

plaining the facts was abandoned. Similar attempts have been made frequently of late, as may be seen by looking into almost any book on thermochemistry, where we find such curves for thermal data. It is obvious that in any not too simple curve singular points will occur. The conclusion is that if we look in this way in such a curve for evidence of the existence of hydrates we shall certainly find it, for every property can be represented by a curve which is usually not very simple (and were it by chance simple, wide conclusions might yet be drawn from it). The peculiar character of such conclusions is that no premisses are required for them.

Prof. Mendelejeff has been very unhappy in his choice of a property to prove the existence of hydrates. The reasons which Ostwald has given, that "specific gravity cannot well be used for setting forth stoichiometric laws," must be considered correct. From the curves which represent the first derived functions of the specific gravity as a function of the percentage composition by weight, Prof. Mendelejeff seeks to deduce the existence and composition of hydrates. This curve for solutions of sulphuric acid, which is given as being particularly instructive, has been twice plotted by Mendelejeff. Below we reproduce the first form it assumes (*Ber. deut. chem. Ges.* 1886, p. 386).

Fig. 1.

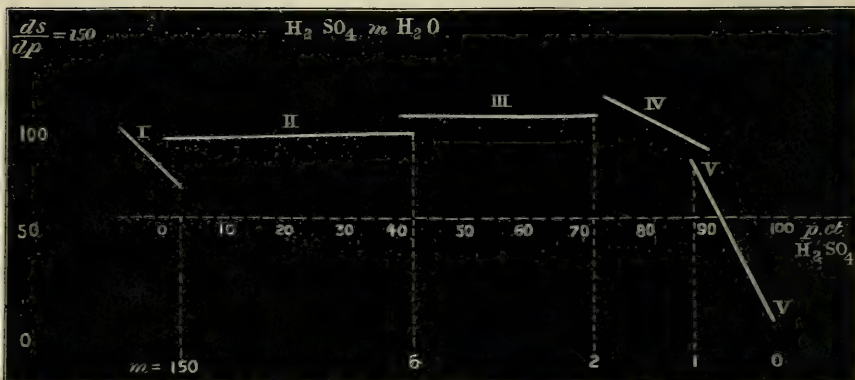


In this figure the empirically obtained numbers are represented. In the second curve (see fig. 2) which is influenced by theory (*Zeitschr. für physikal. Chemie*, i. p. 275) the same numbers are represented in a totally different manner.

Who would be likely to discover that these two curves are identical? Who could recognize the first curve in the straight *Phil. Mag.* S. 5. Vol. 28. No. 170. July 1889. D

lines of the second? And these straight lines have to serve alone as the support of the hydration theory! "*Non dantur saltus in naturâ*" is a proposition which is rightly taken as a motto in every science capable of mathematical treatment.

Fig. 2.



When, exceptionally, a sudden break occurs in a series of phenomena, it must be verified with the greatest care both theoretically and practically before its existence is finally accepted. The first curve therefore must be looked upon as the correct one, because it contains none of those extraordinarily improbable breaks, until the contrary is proved by a thorough investigation such as does not at present exist. We must therefore reject the idea that the existence of the five straight lines of the second curve is proved. Even, however, if this form were correct, still the conclusions which Prof. Mendeleeff draws would be extremely bold ones. For it might be that the straight lines change their position at higher temperatures (as is the case with internal friction), and thus the singular points where they terminate would indicate the existence of quite other hydrates. This, according to a note (*Ber. deut. chem. Ges.* 1886, p. 387), actually occurs with the point of greatest contraction:—"The greatest contraction g in 100 parts by weight at 0° corresponds approximately to $m=3$, but suffers a considerable displacement with rise of temperature, being at 100° near $m=2$." In addition, attention must be drawn to the extreme difficulty of finding the exact positions of these singular points. The experimental material used by Prof. Mendeleeff in his two German publications must be looked upon as insufficient for such purposes.

Strictly speaking, what does the fact that the values of $\frac{ds}{dp}$

are represented by five straight lines signify? Nothing more than that the values of s as a function of p can be represented by five interpolation-formulæ of the second degree with fifteen arbitrary constants, of which Mendelejeff uses only ten. From the mathematical side, this presents nothing more astonishing than that really this number of constants is necessary. There is, we should think, scarcely an example to be found in experimental physics where such an analytical representation has been considered satisfactory. The only conclusion to draw from it is that a much better representation could probably be found with a little trouble. All our modern experience goes to show that we obtain much better results when, instead of percentage composition, we take the number of gram-molecules per litre as abscissæ; and therefore the latter mode of representation is the more scientific. Were this much more justifiable method of plotting adopted the straight lines in the second figure would change into curves, and so the whole foundation of the theory of hydrates would collapse.

Mendelejeff proposes, instead of the dissociation of the electrolytes (MX for example) into ions, a dissociation of $MX \cdot (n+1)H_2O$ into either $MX \cdot nH_2O$ and H_2O , or into $MOH \cdot mH_2O$ and $HX \cdot (n-m)H_2O$ (base and acid) to explain the quantity $i(>1)$. As he himself states, however, the splitting-off of water would not give a sufficient explanation of this fact. We must therefore take the other alternative, that the electrolytes are partially decomposed into acid and base. This decomposition is not conceivable for those electrolytes which are themselves acids or bases, for they could not possibly be decomposed by the action of water. Yet HCl and $NaOH$ have values of i greater than unity. For salts, however, it is at least conceivable. But then we should have to assume that KCl in normal solution had decomposed to the extent of 75 per cent. into KOH and HCl . Now HCl diffuses considerably faster than KOH ; so that if KCl solution were brought into contact with water the latter would become acid from HCl , and the solution alkaline from the KOH remaining behind, just as happens with $FeCl_3$. This is, however, in direct contradiction to all experience, as is every assumption of dissociation (such as Planck has proposed) where the parts with greater velocity do not exercise a strong attraction on those with less, as is the case with electrically charged ions. The view of electrolytic dissociation, on the other hand, is so far from being in contradiction to the facts of diffusion, that the values of the constants of diffusion can be actually deduced from it (*cf.* Nernst, *Zeitschr. für physikal. Chemie*, ii. p. 627). It is surely a strange notion that the pro-

bility of electrolytic dissociation could be in any degree lessened by the possibility of the phenomena which it explains being in the future deducible from other data (*e. g.* the existence of known hydrates). If this were actually to occur, then, conversely, the existence of the hydrates could be deduced from these phenomena, and thus indirectly from the theory of dissociation, so that new territory would be added to the extensive domain already commanded by this hypothesis.

A striking example of this is found in the hypothesis itself. From his powerful generalization of Avogadro's law, van't Hoff had deduced the conditions of equilibrium for several electrolytes in one solvent, and I had done the same from a consideration of the electric conductivity by means of a hypothesis which may be characterized as the imperfectly developed dissociation hypothesis. Immediately after the appearance of this paper by van't Hoff the fusion of the two partially overlapping theories took place, and it cannot be denied that the fruitful period of both was reached only after their union, and was conditioned by this. Although, therefore, the supporters of the dissociation hypothesis cannot homologate the mode of deduction of the views of the great Russian chemist, they have every reason to wish him the best success in his efforts to explain the above-mentioned phenomena.

Leipzig, May 25, 1889.

NOTE.—In the last numbers of the 'Chemical News,' and of the 'Abstracts of the Proceedings of the Chemical Society,' are reports of a paper by Mr. S. U. Pickering which confirms in the most decisive manner the views I have expressed above. Mr. Pickering (*Chemical News*, May 27, 1889, p. 278) says that "on plotting out the first differential of his density-results, he was surprised to find that it formed an irregularly curved figure, and not the rectilinear figure given by Mendelejeff; and he was still more surprised that, on plotting out the values used by Mendelejeff himself, the figure obtained was curvilinear like his own, and not rectilinear like Mendelejeff's. . . . Mendelejeff's statement, therefore, that he had proved the hydrate theory by showing that the densities differentiated into straight lines meeting at points corresponding to definite hydrates is erroneous." He then proceeded to a second differentiation, but "owing to the magnitude of the experimental error" did not take the values of $\frac{ds}{dp}$ obtained directly from his observed results, but instead took "the smoothed first differential curve" as his point of departure (*Proc. Chem. Soc.* May 16, 1889, p. 89).

In this way Mr. Pickering found that the second derived

function (d^2s/dp^2) consisted of straight lines. Mathematically interpreted, this means that in the ds/dp curve angular points or sudden changes of curvature occur. If Mr. Pickering had "smoothed" his curve properly he would evidently have removed these angular points or sudden changes of curvature, for a very small fraction of the "experimental error" would suffice for this purpose. The result can scarcely be gratifying to the supporters of the theory of hydration. Mr. Pickering finds that d^2s/dp^2 is made up of no less than 17 straight lines corresponding to 16 hydrates. In other words, the specific gravity can be represented in the form of 17 equations of the third degree with 68 arbitrary constants, besides the 16 arbitrarily chosen points where the curves begin and end!

This really has very much the look of a *reductio ad absurdum*. The mode of representation entirely lacks experimental foundation, as Mr. Pickering himself tacitly admits in the words "owing to the magnitude of the experimental error." It is characteristic also that Mr. Pickering "agrees with Mr. Crompton's conclusion that they (the d^2k/dp^2 curves; k =conductivity, p =per cent. by weight of sulphuric acid) give a rectilineal figure, but he differs from him in some of the details as to where the breaks occur" (p. 88). But the points "where the breaks occur" should correspond to definite hydrates. The fact is that Mr. Pickering with his multitudinous arbitrary constants can fix the points "where the breaks occur" just where he chooses, and so we need not wonder that the curve for d^2k/dp^2 can be drawn in such a manner "that these breaks agree very closely with those shown by his own density-results" (p. 88).

I will quote in addition a very instructive statement of Mr. Crompton's (Proc. Chem. Soc. Dec. 1888, p. 127):—"Mr. Crompton, replying to Dr. Morley's objection that there did not seem to be any reason why a limit should be put to the differentiation when that had been performed twice, and that it would be just as reasonable to proceed with a third or fourth differentiation and so on, said that a limit to the differentiation would necessarily have to be made according to the nature of the case under investigation and the *discretion exercised by the investigator*. In the present instance the limit of differentiation is clearly indicated by the agreement of the results obtained with those previously arrived at by Mendelejeff by discussing a totally different physical property." But now that Prof. Mendelejeff's results are proved to be "erroneous," we should perhaps expect that the differentiation ought to be carried a little further. This, however, is not necessary, as most of the physical properties can only be determined with

such exactness that the second derived function may be represented, within the errors of observation, by a not too small number of straight lines with practically arbitrary terminations. The proof of this is furnished by the fact that Mr. Pickering has deduced from the specific gravity quite different hydrates (singular points) from Mendeleeff, and from the electric conductivity quite different hydrates from Crompton.

Mr. Pickering closes as follows: "The conclusion is the absolute rejection of any other than the hydrate theory" (p. 89). Looked at from the mathematical point of view the conclusion might well be the "absolute rejection" of the so-called theory of hydrates, at least in the form defended by Mr. Pickering.

V. *On the Molecular Weight of Caoutchouc and other Colloid Bodies.* By J. H. GLADSTONE, *Ph.D., F.R.S.*, and WALTER HIBBERT, *F.I.C.**

DURING the last meeting of the British Association at Bath, we gave a preliminary account of some attempts to determine the molecular weights of caoutchouc and a few other substances by Raoult's method. We have since repeated most of the experiments and largely extended the inquiry, and it seems to us that the results have a certain physical as well as chemical interest.

It is evident that this method is the only one that offers much hope of success in dealing with such substances as caoutchouc, but it is open to question how far the method itself is to be trusted for giving the correct molecular weight of compounds of this description. Our confidence in it, however, was strengthened by the following experiments, made on substances of the same ultimate composition ($nC_{10}H_{16}$), but of known molecular weight in the gaseous condition. We also made experiments on one or two closely allied bodies containing oxygen.

The compounds were dissolved in benzene which had a freezing-point of $5^{\circ}25$ C., and the experiment was conducted in the usual manner. Each degree of the thermometer scale was divided into twentieths, and it was not difficult to estimate to the hundredth of a degree. Successive observations of a freezing-point nearly always agreed to less than $0^{\circ}02$.

The following table gives:—in col. II. the recognized molecular formula, in col. III. the strength of solution, in

* Communicated by the Physical Society: read May 25, 1889.

col. IV. the amount of depression, and in col. V. the molecular weight calculated by Raoult's formula $M = \frac{T}{A}$, where T is the molecular depression constant (in this case = 49), and A is the depression given by 1 gram of the substance in 100 grams of solvent. These figures may be compared with col. VI., which gives the molecular weight deduced from the formula in col. II.

Substance.	Col. II.	Col. III.	Col. IV.	Col. V.	Col. VI.
		per cent.	°		
Oil of Turpentine.....	$C_{10}H_{16}$	4.56	1.59	140.5	136
Oil of Lemon.....	"	6.04	2.17	136.4	136
"	"	3.06	1.12	133.8	"
Cedrene	$C_{15}H_{24}$	3.89	1.00	190.8	204
"	"	4.71	1.20	192.3	"
oprene	C_5H_8	3.30	2.25	71.9	68
"	"	2.20	1.52	70.9	"
Caoutchene	$C_{10}H_{16}$	5.38	2.01	131.1	136
Heveene.....	$C_{20}H_{32}$	12.00	2.32	275	272
"	"	9.37	1.85	248	"
"	"	7.68	1.53	246	"
Camphor	$C_{10}H_{16}O$	4.69	1.59	144.5	152
Menthol.....	$C_{10}H_{20}O$	3.21	0.93	169.1	156
"	"	4.93	1.31	184.4	"
"	"	3.75	1.07	171.7	"
Anethol	$C_{10}H_{12}O$	3.71	1.29	141.0	148

This table shows not merely that the method is applicable in the case of bodies of this description, but that the molecular weights of the liquids in solution have the same relative values as in the gaseous condition.

We then made experiments on caoutchouc, whose empirical formula as usually given ($C_{10}H_{16}$) would indicate a molecular weight of 136, and we found that this was very far below that deduced from our results, as shown in the following table:—

Substance.	Weight in 100 grams of Solvent.	Depression.	Molecular Weight.
Caoutchouc (a) ...	3.1	Scarcely observable.	Extremely high.
" (b) ...	8.8	"	"
" (c) ...	14.6	0.11	6504

The caoutchouc used in solution (a) had been prepared from Penang rubber, by the process described in our previous

paper (Chem. Soc. Journ., July 1888, p. 679). That in solution (*b*) was obtained from Para rubber, by dissolving it in ether, and precipitating the ethereal solution with alcohol. Solution (*c*) was prepared from (*b*) by evaporation in a current of hydrogen. The greater depression observed can hardly be ascribed solely to the greater strength of the solution, since that would only give a proportionate effect. We are inclined to think it possible that there was a lowering of the molecular weight during a three days' gentle heating which was incidentally necessary. The observation, in fact, seems in harmony with other alterations in physical properties which we have sometimes noticed.

This very high molecular weight for caoutchouc strengthens a previous impression of ours that caoutchouc belongs to the class of substances known as colloids. The impression arose from the fact that caoutchouc is a substance showing not the least tendency to crystallize, which cannot be distilled without decomposition, which is subject to great alteration of properties by the action of heat, which is converted into an insoluble modification by small quantities of certain reagents, and which dissolves in its solvents in an extremely sluggish manner.

Graham, in his classic memoir on the subject* of Colloids, observed that "the equivalent of a colloid appears to be always high;" and he also suggested that the colloid molecule may be "constituted by the grouping together of a number of crystalloid molecules."

It seemed worth while therefore to examine bodies commonly regarded as colloidal by Raoult's method. The following table gives the results obtained with aqueous solutions of organic colloids, the molecular weights being reckoned for the ordinary value for *T* given by Raoult in the case of water:—

Substance.	Weight in 100 grams of Solvent.	Depression.	Molecular Weight. <i>T</i> = 19.
Gum arabic	31.6	0.3	2001
Ditto purified ...	14.0	0.165	1612
Caramel.....	8.76	0.105	1585
„	22.5	0.245	1745
Albumen	2.0	Scarcely observable.	Extremely high.

* Phil. Trans. 1861, pp. 183-224.

The molecular weight of these known colloids, as determined by Raoult's method, is very high and confirms the generalization of Graham.

Experiments have already been made upon the so-called carbohydrates by this process by Messrs. H. T. Brown and G. H. Morris*. They found that the sugars had a molecular weight agreeing with the received formula, but the noncrystallizable bodies like soluble starch &c. gave them results suggestive of specially high molecular weight.

We may also note that in some recent investigations by C. Lüdeking, he found that the addition of colloids to water makes no practical difference to the boiling-point, and in every case lowers the vapour-pressure very slightly†. These results all indicate the same general conclusion.

Our experiments were extended by making an examination of solutions of the colloidal hydrates of aluminium and iron. They were prepared by dialysing solutions of the basic chlorides, but, as is well known, a small proportion of the salt must be retained in order to prevent coagulation. The iron solutions contained almost exactly one molecule of chloride to fifteen molecules of the hydrate. The first aluminium solution contained one molecule of the chloride to five or six of the hydrate, the second one of chloride to nearly ten of the hydrate.

Substance.	Weight in 100 grams of Solvent.	Depression.	Molecular Weight. T=47.
Ferric Hydrate	1.16	About 0.01	5452
„	2.60	0.025	4888
Aluminic Hydrate...	0.523	0.060	409.6
„	1.37	0.06	1073.0

The figures here given for the molecular weights of the hydrates are calculated as if the whole depression were due to the hydrate in solution, but the chloride present must have exercised a considerable influence, especially in the first aluminium solution. If allowance be made for this, the molecular weights found would be higher than those given in

* Chem. Soc. Journ. 1888.

† *Ann. Phys. Chem.* [2] xxxv. pp. 552-557.

the table, and would point to the soluble colloidal hydrates of iron and aluminium being many multiples of $\text{Fe}_2\text{H}_6\text{O}_6$, or $\text{Al}_2\text{H}_6\text{O}_6$, which would give a molecular weight of only 214 and 157 respectively. The molecular weights of ferric and aluminic chlorides, as determined by Raoult's method (T being 47), are about 114 and 106 respectively.

All our experiments, therefore, while affording additional illustrations of the value of Raoult's method, confirm the belief that the molecule of a colloidal substance is an aggregate of a very great number of atoms*.

VI. Water-spray Influence-Machine.

By GEORGE FULLER †.

THIS machine is for obtaining directly from a fall of water a supply of electricity of a high potential. It consists of four similar parts arranged symmetrically round a central vertical support, and each division has the following members.

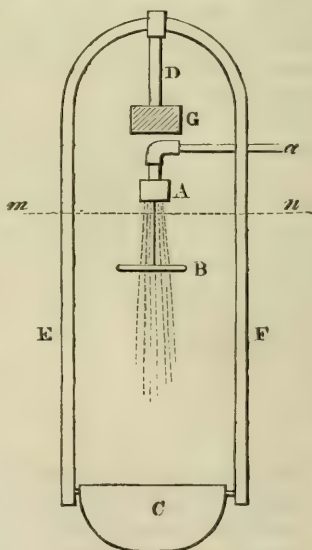
A nozzle, A, in connexion with a head of water by means of a pipe, *a*.

A ring, B, of brass or copper wire placed vertically below A, and through which the water descends when the machine is in action.

A vessel, C, placed below B to receive the water that has passed through the ring.

A brass tube, E F, between the ends of which the vessel C can turn about a horizontal axis.

An insulating glass rod, D, to the top of which the tube E F is attached, and with the lower end fixed in a part of the frame of the machine, G.

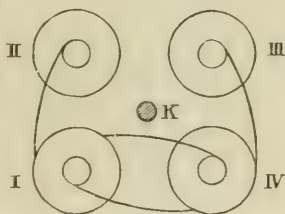


* Since this paper was read we have found that Paternò and Nasini have arrived at the same conclusion from experiments on albumen and gelatine (*Lincci*, April 7, 1889, p. 476).

† Communicated by the Physical Society: read May 25, 1889.

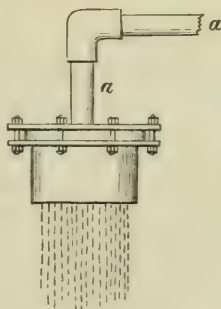
A sectional plan on *mn* shows the connexion between the four divisions, which are numbered I., II., III., IV. The wire ring of section I. is in electrical connexion with the receiver of section IV. Similarly the ring of II. is connected with I., the ring of III. with IV., and that of IV. with I.

K is a central column for supporting four arms of the machine to which are fixed the insulators D.



The discharge of electricity is taken between conductors in connexion with II. and III.

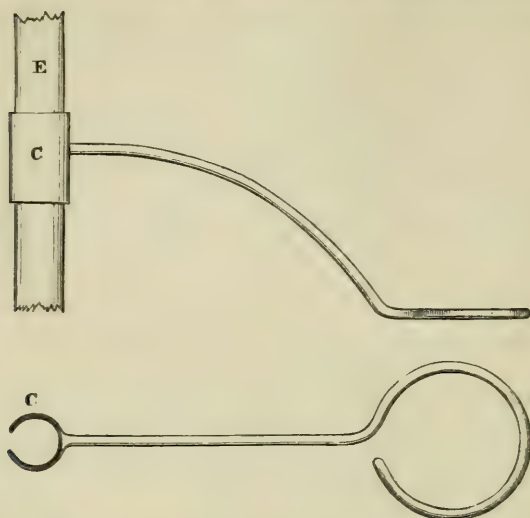
The nozzle is a flanged brass box, the bottom of which is perforated with small holes through which the water descends. It is fixed by a number of small bolts and nuts through its flange to a brass plate fixed to the supply-pipe, *aa*, and the joint is made watertight by a vulcanized india-rubber ring. A piece of fine linen covers the top of the box to strain the water before it reaches the small holes, as it was found that the water was either stopped or diverted by small particles unless this precaution was taken. The holes, which are circular, have a diameter of $\frac{1}{1000}$ ''; as it was found that when holes $\frac{7}{1000}$ '' were used the water was so much dispersed by the working of the machine that a great part of the water ceased to fall into the receivers. This great dispersion also injured the insulation, and besides this it was extremely difficult to keep these holes free.



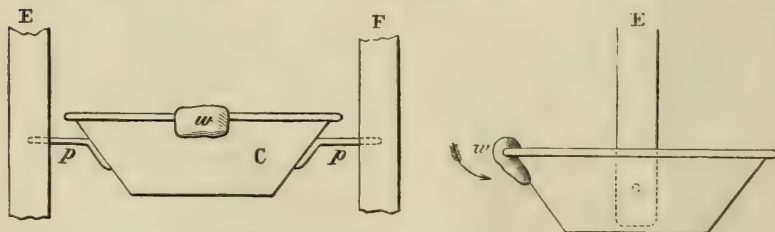
With regard to the number of holes. In the nozzles of sections I. and IV. there are six arranged in a circle of $1\frac{1}{4}$ '' diameter. For those of sections II. and III., either a pair with twelve holes each in a circle of $1\frac{1}{4}$ '' diameter, or with eighteen holes in a circle of $1\frac{3}{4}$ '' diameter.

The rings are made of brass or copper wire of about $\frac{1}{8}$ '' diameter. The inside diameter of the rings used with the nozzles with six and twelve holes is $2\frac{1}{4}$ '', and with the eighteen holes $2\frac{3}{4}$ ''.

The wire of each ring is continued and fixed to a clip of split brass tube, C, which slides upon the brass tube E or F.



This enables the depth of the ring below the nozzle to be adjusted, which is of importance, as the greater the head of water employed the greater must be the distance between the two, as the ring should be fixed at the point where the small streams of water break up into spray. The receiving vessel, C, may be of glass or metal, as the former material, from its constant state of moisture whilst the machine is working, seems to conduct the electricity as effectually as the latter. In the author's model they were at first of glass, but one of them having been broken they were replaced by receivers of zinc. The receivers are supported by pins, *p, p*, which are soldered to them at one end, whilst their free ends rest in holes drilled in E and F. To make the receiver self-acting they are hung so that when a leaden weight, *w*, is fixed, as



shown in sketch, the vessels being empty, they would turn in the direction of the arrow if they were not prevented by stops

soldered to them which press against E and F; but when the receivers are nearly full of water, their balance is such that they turn in the opposite direction and so empty themselves. The four receivers are made to turn towards the axis of the machine and to deliver the water into a metal bath, which for continuous action should be connected with a drain.

The following are some of the dimensions of the author's model :—

Zinc receivers 8'' diameter at the top.

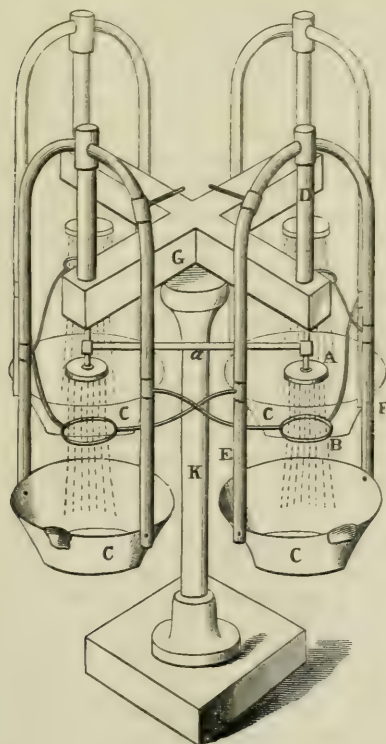
Brass tube E and F $\frac{3}{8}$ '' diameter.

Glass rod D $\frac{3}{4}$ '' diameter, with an insulation of 4''.

From the rim of C to the lower surface of A, $9\frac{1}{4}$ ''.

From the rim of C to the underside of stand, 1' 1''.

From centre to centre of insulators D across the centre line of instrument, 1' 1''.



The Electrical Action of the Machine.

An instrument made of only sections I. and IV., with their rings connected with their receivers, as shown above, will charge itself; and the difference of potential of the two receivers may be such that sparks $\frac{1}{2}$ inch long may occasionally pass between them, though more usually $\frac{3}{8}$ inch is the longest that can be obtained with a head of water of about 23 feet. With this arrangement, after every discharge the potential of the rings is nearly equalized; whereas in the machine with four sections, I. and IV. keep up the difference of potential of the rings of II. and III.

With respect to the action of the machine, the author, whilst giving the considerations from which it was constructed, must leave to the electrician to determine whether they have anything to do with the true explanation of the phenomena. The water, at the point where it is divided into drops by the resistance of the air, is electrified by induction from the rings; the former being in connexion with the earth through the unbroken water of the stream, and the action seems similar to that employed in Sir W. Thomson and Professor Silvanus Thompson's water-dropping accumulators. That such is the case appears to follow from the fact that, if the rings are either placed much above or much below the level where the water breaks into spray, the machine ceases to work. When the rings are at their proper level there is an additional action; for the particles that are inductively electrified are split up into numberless minute particles, some of which are so fine that they float about in the air and do not fall into the receiver. And it is this breaking up of the water into minute particles that the author thinks may account in part for the effect produced; for when a number of spheres that have been electrified unite into a mass of less surface, their potential in the latter state is higher than in the former.

Another point which the author thinks must be taken into consideration is the speed with which the particles move through the ring, as it was only when he experimented with a fall of some feet instead of inches that he obtained a potential high enough to produce sparks. With a very slow speed the attraction of the ring is too strong for the water, so that it at last, as in Sir William Thomson's apparatus, bends against it. That the division of the drops into minute spray plays a part in the action of the machine seems to be shown by the fact that sparks of the same length, in the same state of the atmosphere, have been obtained from it when the velocity of the water has been very much diminished. The sparks, as a rule, have not been so numerous per minute, but

the water has been divided into finer spray. At times, even with half the delivery of water, the same length of spark has been obtained.

One experiment the author has made in which the spray was not obtained by the action of gravity, but by a steam "atomiser," as it is called. The water and steam passed through a copper wire ring $1\frac{1}{2}$ " diameter, connected with one of the receivers of an apparatus made up of sections I. and IV., as above. The nozzle was $\frac{5}{8}$ " from the ring and $5\frac{5}{8}$ " from the receiver. Sparks $\frac{1}{4}$ " in length were taken freely from the receiver, which is a better result than has been obtained with a fall of water of some 23 feet. What was very observable in this case was the very small amount of water used, a small teacup-full being passed over in some five or six minutes; and the author has recorded in his notes that the experiment was made on a very wet day.

Adding to the number of jets does not seem to increase the power of the machine, either in quantity or potential, at all in proportion to the number added; though the action of an electrical machine is so eccentric that it is difficult to be certain of this, for at times the nozzle with eighteen jets has given much better results than the one with twelve jets.

It has been stated that, in the machine as made, the rings are 1" larger in diameter than the circle of the jets, and it is found that they give a better result than when larger rings are used; but in some experiments with a small flow of water a ring $3\frac{1}{2}$ " diameter gave as large a spark as one of $1\frac{1}{2}$ ". In the dark, electricity is often seen to fly off from the rings, the water on them being made into pointed-shaped drops.

The machine in its present form is by no means powerful, as with a small Leyden jar attached to it the longest spark has hitherto been $1\frac{1}{8}$ ", the head of water being about 23 feet. The state of the atmosphere has very great influence on the working of this machine; for though in all states of the weather electricity will be generated, it requires a fairly dry atmosphere to give 1" sparks.

It may be mentioned that the machine has only been tried in a small bath-room, which is a very unfavourable place for electrical experiments; and it perhaps is worth mentioning, that on one occasion sparks were only obtained when window and door were open and the machine was in a thorough draught.

To what extent the power of the machine may be increased it is difficult to predict; but the author thinks that the experiment with the atomiser points to high velocity in the water, combined with minute subdivision, as the direction in which any future attempts should be made.

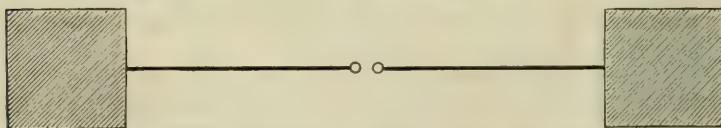
VII. *On Electric Radiation and its Concentration by Lenses.*
 By Prof. OLIVER J. LODGE, *D.Sc., F.R.S.*, and JAMES
 L. HOWARD, *D.Sc.**

Introduction by Dr. Lodge.

IN making exact optical experiments on electric radiation it is necessary to be able to converge it and throw a beam of it in any desired direction. To do this by means of mirrors is possible, but not always very convenient. Prof. Fitzgerald and Mr. Trouton† have related the difficulty they at first found in making concave mirrors work; and we experienced the same difficulty, intensified probably in our case by the fact that we tried to work with everything on an extra small scale—half the linear dimensions of Hertz.

It is much easier to work with a large oscillator than a small one, because the same extraordinary suddenness in starting the oscillations is not then essential; only with large waves, mirrors and everything have to be heroic to match, and our laboratory was not big enough for optical experiments on gigantic waves. Electrical experiments on such waves I have made in large numbers, obtaining them originally by means of discharging Leyden jars, but recently sometimes by a gigantic Hertz oscillator consisting of a pair of copper plates, each consisting of a couple of commercial sheets soldered together and rimmed round with wire, connected by a length of No. 0 copper wire interrupted in the middle by a couple of large knobs. The plates and connecting-rod are hung from a high gallery, so that everything occupies one plane, their distance and dimensions being here shown.

Fig. 1.—Large Oscillator used for violent and distant effects. Scale $\frac{1}{80}$.



Plates 120 centim. square. Knobs 3.2 centim. diameter.

Each rod 230 centim. long and 8 millim. diameter.

Spark-gap about 1.5 centim.

* Communicated by the Physical Society: read May 11, 1889.

† 'Nature,' vol. xxxix. p. 391.

Static capacity, $\frac{S}{K} = 25$ centim.

Self-induction, $\frac{L}{\mu} = 8320$ „

Characteristic factor, $\log \frac{4l}{a} = 7.9$.

Rate of vibration, 10 million per second.

Wave-length, 29 metres.

Dissipation-resistance, 22,500 ohms.

Initial stock of energy, about 300,000 ergs.

Power of initial radiation, 128 horse-power.

Number of vibrations before energy would be *at this rate* dissipated, about 3.

The electrical surgings obtained while the Hertz oscillator is working are of just the same character as are noticed when a Leyden jar is discharging round an extensive circuit; but whereas from a closed circuit the intensity of the radiation will vary as the inverse cube of the distance as soon as the circuit subtends a small angle, the radiation from a linear or axial oscillator varies in its equatorial plane only as the inverse distance, as Hertz showed.

Hence, for obtaining distant effects the linear oscillator is vastly superior. Its emission of plane-polarized, instead of circularly-polarized, radiation is also convenient.

(I may mention that a thundercloud and earth joined by a lightning-rod or by a disruptive path constitute a linear oscillator; and hence radiation-effects and induced surgings may be expected to occur at very considerable distances from a lightning-flash.)

Exciting this oscillator by a very large induction-coil, extraordinary surgings are experienced in all parts of the building, and sparks can be drawn from any hotwater-pipe or other long conductor, whether insulated or otherwise, and from most of the gas-brackets and water-taps in the building, by simply holding a penknife or other point close to them. From conductors anywhere near the source of disturbance the knuckle easily draws sparks.

Out of doors some wire fencing gave off sparks, and an iron-roofed shed experienced disturbances which were easily detected when a telephone-terminal was joined to it, the other terminal being lightly earthed. [Sometimes I utilized the wire fencing as one of the plates of the oscillator, and thus got still bigger and further spreading waves.]

The waves thus excited are from 30 to 100 yards long, and optical experiments with them would be as difficult and vague as are experiments on sound-waves of corresponding length. Small oscillators can, however, easily be employed which shall

give waves from a foot to a yard in length ; and after reading Hertz's experiment of the pitch prism*, I made preparations for casting some great lenses that should give, I hoped, easy concentration of such waves.

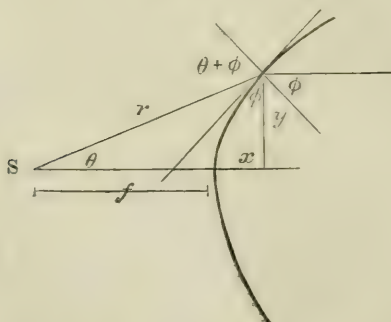
Paraffin was a natural substance to use ; but it is rather expensive, and has not a very high index. After considering many substances—beeswax, sulphur, &c.—I decided to try resin, and laid in a stock of that substance. Meanwhile, to gain experience in casting, and finding that a common class of pitch could be obtained at an absurdly low price, I procured several casks of the commonest pitch also. I did not contemplate using this substance at first because I feared it would be an imperfect insulator, and there seemed no use in permitting any dissipation of energy whatever, so long as one could get perfectly transparent substances.

On casting a specimen of the pitch, however, it was found so strongly insulating as nearly to fling off the leaves of a gold-leaf electroscope it was brought near. It seems, therefore, an excellent cheap stuff for electrophorus and such like use, wherever it is not expected to be strictly solid ; and it can hardly help being transparent except to very little waves.

Meanwhile we had calculated that to receive rays from one point and convey them to another without aberration, a pair of plano-hyperbolic lenses were very suitable ; a parallel beam being transmitted from one lens to the other. The lenses would naturally be made cylindrical, instead of spherical, to suit the linear form of radiator.

The optical calculation of a lens free from aberration for

Fig. 2.



one special point, S, from which it is to receive rays and emit

* Wied. *Ann.* xxxvi. p. 769 (1889) ; translated in *Phil. Mag.* April 1889.

them parallel, is as follows :—From fig. 2,

$$\mu = \frac{\sin(\theta + \phi)}{\sin \phi} ; \quad y = (x + f) \tan \theta ; \quad \frac{dy}{dx} = \cot \phi.$$

Solving these equations, and making θ and x vanish together, we get, as the curve of the lens,

$$r = (x + f) \sec \theta$$

or

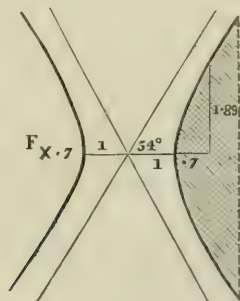
$$r = \frac{f(\mu - 1)}{\mu \cos \theta - 1} ;$$

a hyperbola with one focus as origin, with eccentricity μ , semi-latus rectum $f(\mu - 1)$, and semi-axis major $\frac{f}{1 + \mu}$.

Taking μ as 1·7 for pitch (according to the measurement of Hertz with a prism), and calling the semi-axis major unity, the focal length of the lens is 2·7, the semi-latus rectum 1·89, and half the angle between the asymptotes, being $\cos^{-1} \frac{1}{\mu}$, is 54° .

Using these data, and taking six inches as unit of length, a curve was drawn as shown in fig. 3, where F, the focus of the hyperbola, is to be also the principal focus of the lens ; its distance from the lens is 41 centim.

Fig. 3.



This curve was given to the laboratory assistant, Mr. Davies, who cut out a pair of wooden templates to the pattern, nailed sheet zinc to them so as to make a mould, propped it up in an outhouse, and proceeded to cast it full of pitch—the upper fluid surface constituting the plane surface of the lens.

All went well till the mould was nearly filled, when the

weight of the pitch ripped the zinc from its fastenings, and a horrid collapse was the result.

A couple more moulds were made of the same pattern, only stronger, and a bed of sawdust and mould was made to sustain the weight. A double partition of thin wood was introduced across the middle of the mould, so as to enable each lens to be split into two halves if it should happen to be too immovable in one piece.

After a time two satisfactory lenses were obtained, each nearly a metre square. Nothing could be done with them during term, because of want of space; but in the Easter holidays I requested my demonstrator, Dr. Howard, to make experiments in one of the College corridors. There exists a large open room or iron shed, which I should have preferred to use; but unfortunately dry rot had set in in its flooring, and it was in the hands of the carpenters all vacation. We are therefore somewhat troubled by neighbouring walls and by hotwater-piping.

Under more favourable circumstances, the distance between the lenses might no doubt have been much greater; in fact, no attempt was made to place the lenses far apart. They were set up with their flat faces parallel at the opposite ends of a table, about 6 feet apart, and not afterwards moved, being, indeed, rather unwieldy; the oscillator was placed in the principal focus of one lens, viz. at a distance of from 41 to 51 centim. from its curved surface. The focal length, calculated on the assumption that $\mu = 1.7$, was 41 centim., but experimentally 51 centim. seemed to do better.

After the few experiments here recorded were done, one of the lenses took advantage of Easter week to assert its essential fluidity, and so much bulged and curved over as to be almost unserviceable; since then it has completed its ruin by breaking its prop and tumbling over into fragments. The other lens stands remarkably well, and seems as good as ever. There is evidently an important difference in the quality of the pitch, though it is not a difference recognized by the invoice. On the whole I think paraffin would have been the best substance to use.

The particular form of receiver is a comparatively unimportant matter, but I prefer linear ones to circular or nearly closed circuits as being more sensitive at great distances, for much the same reason as has been stated for oscillators.

Exact timing of the receiver is unessential. If resonance occurred to any extent, so that the combined influences of a large number of vibrations were really accumulated, the effects might doubtless be great; but hitherto I have seen no evi-

dence of this with linear oscillators ; the reason being, I suppose, that the damping out of the vibrations is so vigorous that all oscillations after the first one or two are comparatively insignificant ; and very bad adjustment, or no adjustment at all, will give you the benefit of all the resonance you can get from such rapidly decaying amplitudes. The main reason of the rapid damping is loss of energy by radiation. The "power" of the radiation while it lasts is enormous, and the stock of energy in a linear oscillator is but small.

Leyden-jar discharges in closed circuits die away more slowly, and for them some approach to exact timing is essential, if a neighbouring circuit is to respond easily.

In working with small oscillators it is essential that the spark-knobs shall be in a state of high polish, else the sparks will not be sufficiently sudden to give the necessary impetus to the electrification of the conductors.

Any hesitation or delay about the spark permits the potentials of the knobs to be equalized by a gradual subsidence which is followed by no recoil, just as a tilted beer-barrel may be let down gently without stirring up the sediment by waves. The period of a natural vibration is comparable to the time taken by light to travel a small multiple of the length of the oscillator, and hence not a trace of delay is permissible in the discharge of a small conductor if any oscillations are to be excited by means of it. Thus if an electrostatic charge on a conducting sphere be disturbed in any sudden way, it can oscillate to and fro in the time taken by light to travel 1·4 times the diameter of the sphere, as calculated by Prof. J. J. Thomson ; and hence it is by no means easy to disturb a charge on a sphere of moderate size except in what it is able to treat as a very leisurely manner. Even on large spheres the oscillations cannot be considered slow : thus an electrostatic charge on the whole earth would surge to and fro 17 times a second. On the sun an electric swing lasts $6\frac{1}{2}$ seconds. Such a swing as this would emit waves 19×10^5 kilometres or twelve hundred thousand miles long, which, travelling with the velocity of light, could easily disturb magnetic needles* and produce auroral effects, just as smaller waves produce sparks in gilt wall-paper, or as the still smaller waves of Hertz produce sparks in his little resonators, or, once more, as the waves emitted by electrostatically charged vibrating atoms excite corresponding vibrations in our retina. It may be worth while to suspend at Kew a compass-needle with a natural period of swing of 6·6 seconds, and see whether it resounds to solar

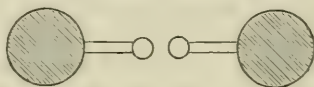
* Cf. Mr. Oliver Heaviside, *Phil. Mag.* February, 1888, p. 152.

impulses. Another, but almost microscopic, recording needle with a period of $\frac{1}{17}$ second might also be suspended.

The charge on the oscillator used in the present set of experiments vibrates 300 million times a second, which, though slower than the electric quiverings on, say, a three-inch ball, is yet quick enough to demand care and attention.

With very large oscillators, such as that described at the beginning of this paper, no such minute precautions need be taken.

Fig. 4.—Small Oscillator used for optical experiments. Scale $\frac{1}{8}$.



Plates 8 centim. diameter.

Knobs 2 centim. diameter.

Each rod 6 centim. long and 1 centim. diameter.

Spark-gap about 8 millim.

Static capacity, $\frac{S}{K} = 1.4$ centim.

Self-induction, $\frac{L}{\mu} = 190$ „

Characteristic factor, $\log \frac{4l}{d} = 4.5$.

Rate of vibration, 300 million per second.

Wave-length, 1 metre.

Dissipation-resistance, 7250 ohms.

Initial stock of energy, about 5400 ergs.

Power of initial radiation, 128 horse-power.

Number of vibrations before energy would be *at this rate* dissipated, about $1\frac{1}{2}$.

My oscillator is a good deal dumper, and its ends have more capacity, than those of corresponding wave-length used by Hertz ; the reason being that I prefer to make the electrostatic capacity bear a fair relation to the electromagnetic inertia, so as to gain a reasonable supply of initial energy for radiation. The store of energy is proportional to the capacity ; the rate at which it is radiated per second is independent of it. Large terminal capacity helps to preserve a high potential longer, and so prolongs the duration of the discharge.

The wave-length of the emitted radiation is easily calculated approximately from the expression

$$\lambda = 2\pi \sqrt{\left(\frac{L}{\mu} \cdot \frac{S}{K}\right)},$$

where $\frac{L}{\mu} = 2l \log \frac{4l}{d}$; l being the length of the entire rod portion of the oscillator, and d its diameter*. The measurement of l is the most unsatisfactory part. It is best to include the knobs and spark-gap as part of the whole length; the constriction at the spark will increase that part of the self-induction, but the expanse of the knobs will diminish another part. A trifle extra length should be allowed for the currents in the disks or balls at the end; but to measure l from centre to centre is rather too much allowance. From centre of one to nearest point of the other is a fair compromise.

As to S , it will be practically half† the static capacity of the sphere or plate at either end of the oscillator, especially if these are pretty big compared with the size of the rod. Strictly speaking they are not isolated, even when far from other conductors, because they are in presence of each other, but the correction is usually small. For instance, for two oppositely charged spheres of radius r , at a considerable distance l from centre to centre, the capacity is about

$$\frac{\frac{1}{2}lr}{l-r} = \frac{1}{2}r \left(1 + \frac{r}{l}\right).$$

Hence the ordinary value of the capacity, as recorded for convenience below, is always a minimum which circumstances may increase but hardly diminish.

Values of $\frac{S}{K}$ for Isolated Bodies.

For a globe, its radius.

For a thin circular disk, $\frac{2}{\pi}$ times its radius.

For a thin square disk, 1.13 times inscribed circular disk,
or .36 times a side of the square.

For a thin oblong disk, a trifle greater than a square of the
same area.

Intensity of the Radiation.—Hertz has shown‡ that the amount of energy lost per half swing, by a radiator of length l charged with quantities $+Q$ and $-Q$ at its ends respectively, is

$$\frac{\pi^4 Q^2 l^2}{3K(\frac{1}{2}\lambda)^3}.$$

* See Addendum at end.

† *Half*, because the two spheres are technically "in series." See Addendum at end of paper.

‡ Wied. *Ann.* January 1889; or *Nature*, vol. xxxix. p. 452.

He omits the dielectric constant K , because he supposes Q expressed in electrostatic units, but it is better to make expressions independent of arbitrary conventions.

So the loss of energy per second, being $\frac{v}{\frac{1}{2}\lambda}$ times the above, is

$$H = \frac{16\pi^4(Ql)^2v}{3K\lambda^4};$$

and this therefore is the radiation power.

For a given electric moment, Ql , the radiation intensity varies therefore as the fourth power of the frequency, *i. e.* inversely as the fourth power of the linear dimensions of the oscillator, as Fitzgerald some time ago pointed out.

But inasmuch as different oscillators will not naturally be charged to the same electric moment, but will rather be charged to something like the same initial difference of potential, as fixed by the sparking interval between their knobs, it will be better to write $Q=SV$, and to insert the full expression for λ .

Doing so, we get for the radiation activity at any instant when the maximum difference of potentials at the terminals is V ,

$$\begin{aligned} H &= \frac{\pi^4 S^2 V^2 l^2 v}{3\pi^4 K S^2 L^2 v^4} = \frac{V^2}{3K\mu^2 v^3 \left(2 \log \frac{4l}{d}\right)^2} \\ &= \frac{V^2 K v}{12 \left(\log \frac{4l}{d}\right)^2} = \frac{V^2}{12\mu v \left(\log \frac{4l}{d}\right)^2}; \end{aligned}$$

an expression roughly almost independent of the size of the oscillator. Quite independent of it if the length and thickness of its rod portion are increased proportionately.

(The factor μv may always be interpreted as 30 ohms whenever convenient.)

Thus all oscillators, large and small, started at the same potential, radiate energy at approximately the same rate; short stout ones a little the fastest.

But the initial energy of small oscillators being small, of course a much greater proportional effect is produced in them, and the radiation ceases almost instantaneously, their energy being dissipated in a very few vibrations. On the other hand, oscillators of considerable capacity keep on much longer; and with very large ends, as in Leyden jars, the loss

of energy by radiation is often but a small fraction of that turned into heat by the frictional resistance of the circuit.

The expression for the radiating power may be compared either with the form $\frac{1}{2}SV^2$ or with the form $\frac{V^2}{R}$; and the loss of energy may be said to be like a static capacity of

$$\frac{30 \text{ earth quadrants}}{6 \left(\log \frac{4l}{d} \right)^2}, \text{ or } \frac{5556 \text{ microfarads}}{\left(\log \frac{4l}{d} \right)^2},$$

charged to the potential V , being discharged once a second; or like the heat produced per second in a resistance of $360 \left(\log \frac{4l}{d} \right)^2$ ohms, having a difference of potential V between its ends. The duration of the discharge must therefore be exactly comparable to the time a wire of this resistance would take to equalize the potential of the oscillator-ends initially charged to the same difference of potential.

For the small oscillator used in the optical experiments here recorded, the value of $\log \frac{4l}{d}$ is approximately $4\frac{1}{2}$; hence the equivalent resistance is 7250 ohms. And, since the initial difference of potential is, say, 26,400 volts, the power of the initial radiation is 96,000 watts or 128 horse-power.

At this rate the whole original stock of energy (5400 ergs) would be gone in the two-hundred millionth of a second, *i. e.* in the time of $1\frac{1}{2}$ vibration; but of course the energy really decays logarithmically. The difference of potential at any instant being given by

$$\frac{d(\frac{1}{2}SV^2)}{dt} = \frac{V^2}{R}, \quad \text{that is, } V = V_0 e^{-\frac{t}{RS}};$$

where R is the above 7250 ohms plus the resistance of the spark and of the oscillator itself to these currents. The resistance of the spark is probably but a dozen, or perhaps a hundred, ohms; that of the small oscillator is about $\sqrt{(lr)}$ ohms, where r is its ordinary resistance to steady currents expressed in ohms, and l is its length in centimetres. This, therefore, is utterly negligible; practically the whole of its energy goes in radiation. For the big oscillator the resistance is about $\sqrt{(\frac{1}{30}lr)}$; and so for a linear oscillator in general the dissipation resistance may be considered as simply

$$R = 360 \left(\log \frac{4l}{d} \right)^2 \text{ ohms.}$$

Nothing approaching continuous radiation can be maintained at this enormous intensity without the expenditure of great power, a hundred and thirty horse-power if my calculation is right. Under ordinary circumstances of excitation the intervals of darkness are enormous; if they could be dispensed with, some singular effects must occur. To try and make the radiation more continuous a large induction-coil excited by an alternating machine of very high frequency, or by a shrill spring-break, might be tried. But even if sparks were made to succeed one another at the rate of 1000 per second, the effect of each would have died out long before the next one came. It would be something like plucking a wooden spring which, after making 3 or 4 vibrations, should come to rest in about two seconds, and repeating the operation of plucking regularly once every two days.

Statement of Results by Dr. HOWARD.

The apparatus used consisted of (1) an oscillator, or transmitter, with exciting coil; (2) a resonator or receiver, and (3) two lenses of pitch. We shall describe these in order.

The Oscillator or Source of Radiation.—This was made in two similar halves, each constructed by soldering to one end of a brass rod, 6 centim. long and .95 centim. diameter, a thin circular copper disk of 4 centim. radius. To the other end was soldered a spherical brass knob of 1 centim. radius, highly polished. A small hole was drilled in each rod at a distance of 1.3 centim. from the knob to allow of the insertion of connecting wires to the Ruhmkorff coil by which it was excited. The two disks were cemented to two small wooden blocks which could be clamped in any position on a vertical glass rod. By this means the distance between the knobs could be easily adjusted, and the apparatus could be inclined when wished.

The induction-coil was of the usual pattern with hammer-break. With the current used (supplied by 6 accumulator-cells) it gave a continuous stream of sparks between two points 2.5 centim. apart connected to the secondary terminals. The knobs of the oscillator were usually separated by a space of from .7 to 1.0 centim. They required cleaning about every 20 minutes owing to burning produced by the spark. This burning was always greater at one knob than the other; greatest apparently at the one that mattered least, for if the primary current was reversed after the oscillator had been working some time the intensity of its radiations immediately decreased perceptibly.

The length of a *complete* wave emitted by the oscillator,

calculated from its dimensions after the manner of Hertz, is 100 centim. And this is a sufficient amount longer than the conductor itself for the calculation to be not very inexact. It cannot pretend to accuracy.

The Resonator, or receiver and detector of radiation (the electric eye, as Sir W. Thomson calls it), was of the simplest possible construction. Two pieces of copper wire (No. 13 B. W. G.) were cut each to a length of 25 centim. One end of each was rounded off, and to the other end was attached a small rectangular brass scrap or plate at right angles to the wire. These little plates each carried a point; one of these points was fixed, and the other adjustable by a screw, by means of which the distance between them could be varied. The resonator was fastened to a piece of wood a little longer than itself. Its total length, including points and strips, was 53 centim., *i. e.* about half the calculated wave-length of the oscillator. A better mode of expressing it is to say that each half of the resonator is approximately a quarter wave-length, and corresponds to a closed organ-pipe, or to a resonant column of air in a glass jar.

The lenses were made of common mineral pitch, which was found to insulate quite well enough for the purpose. They were cast in the form of hyperbolic cylinders, bounded by a plane perpendicular to the axes of the principal hyperbolic sections; the eccentricity of the latter was equal to 1.7, and was taken as a fair approximation to the refractive index of pitch for infinitely long waves. A lens of this form should converge a bundle of parallel rays falling normally on its plane surface to a line of foci coinciding with the outer foci of its principal hyperbolic sections; and, *vice versa*, rays proceeding from this focal line and falling on the curved surface should emerge from the lens as a bundle of parallel rays. Hence, if the oscillator be placed along the focal line of one lens, the electric rays from it will be sensibly parallel after traversing the lens, and after falling normally on the plane surface of the second lens should converge and meet at its focal line. The lenses were almost equal in size. Their plane surfaces were nearly square, being 85 centim. high and about 90 centim. broad. The greatest thickness (from vertex of hyperbola to plane surface of lens) is 21 centim. The lenses are each separated into an upper and lower half by means of a thin wooden partition inserted during the casting. It was intended to divide this partition by a saw-cut, and thus allow the lenses (each of which weighs more than 3 cwt.) to be more easily carried about. So far, however, this has not been done.

In making the experiments the lenses were placed one at each end of a wooden table $2\frac{1}{2}$ metres long, with their plane surfaces turned towards each other, and as nearly as possible parallel. The distance between them was 180 centim., and remained the same throughout the experiments. On one side of the table close to the edges of the lenses was a brick wall about 40 centim. thick; and on the other side was a residue of gangway 54 centim. wide between the lens and a laboratory-apparatus cupboard, which has had to be set up in the corridor for want of space elsewhere. The oscillator stood, together with its exciting coil, on a small table whose height was adjustable; the plane of its disks was parallel to the flat surfaces of the lenses in all cases. It was intended to be placed in the focal line of the first lens; but apparently the index of refraction had been assumed too high, and a position 51 centim. from the vertex of the lens seemed to do best. We shall speak of the vertical plane through the focal lines of the two lenses as the "*axial plane*." It contains the axes of the lenses and of the oscillator. Waves seem to be emitted more powerfully in this plane normal to the disks of the oscillator than in the plane containing them.

The direct effect from the oscillator could be perceived by the resonator at a distance of 120 centim. in the axial plane in the most favourable case; that is to say, in a very dark room and just after cleaning the knobs of the oscillator. Under similar circumstances resonance was only just obtainable at the vertical edge of the first lens, viz. 85 centim. from the oscillator in a direction making an angle of about 30° with the axial plane. To get a rough measure of the intensity of the radiation at any point, the resonator was placed there, and its spark-gap arranged so as to just give a continuous stream of sparks; it was then brought to the line joining the oscillator and the edge of the first lens (line of reference), and the distance from the oscillator observed at which the sparks ceased to be continuous. When the intensity of the radiation was very small, however, the converse of this method was adopted; the resonator was adjusted at the line of reference and then taken to the point at which the intensity was to be observed.

The following are the phenomena observed in the space between the two lenses when the oscillator coincides with the focal line of the first one. The resonator gives brilliant sparks in the axial plane near the first lens so long as it is held parallel to the oscillator. On rotating it in a plane perpendicular to the axial plane the sparks decrease in brilliancy and length, and become entirely obliterated when the resonator and oscillator are at right angles. If the rota-

tion is continued the sparks reappear and regain their former brilliancy, when the resonator again reaches its first position. If the resonator be placed in the axial plane and then moved parallel to itself towards the edge of the lens, the intensity of the sparking gradually decreases as we get nearer the edge, and on the side nearest the wall the sparks cease altogether at the edge of the lens. On the other side, however, they are visible right up to the edge of the lens, and then very abruptly cease, when the direct effect alone is obtained. The beginning of the sparking, as soon as the resonator enters the shadow of the lens, is very noticeable. The same appearances are observed at all distances from the first lens, but the intensity of the radiation is, of course, smaller as we get further from the oscillator. The radiations are always a little more feeble on the side nearest the wall than on the other side. The cause of this has not yet been definitely ascertained, but it appears to be produced by some action of the wall itself. Slightly altering the position of the oscillator did not get rid of the effect; so it cannot be due to the oscillator being out of focus. There was apparently no defect in the lens itself which could account for it. The concentration of the radiations by the lens is very well marked. Just after passing through the first lens in the axial plane they are almost as intense as when they first impinge on its curved surface; that is to say, they do not lose appreciably in intensity by traversing the 21 centim. of pitch. But this concentration is even more clearly shown by the fact that in the axial plane, at the surface of the second lens (250 centim. from the oscillator), the sparks are quite as intense as the direct effect would be at 100 centim. in the same plane if the first lens were removed; or, again, the resonator will give sparks easily at the surface of the second lens, and when brought to the line of reference will not give sparks at a greater distance than 70 centim. At the surface of the second lens the irregularity mentioned above is a little greater than at the surface of the first one.

Beyond the second lens the rays are converged, as we expected, and there is a fairly well defined point in which they meet; but the intensity of the sparking at the focus of the second lens is not appreciably greater than at its surface. Probably this is due to the fact that the rays from the edge of the lens, having travelled a much longer distance in air than those in the axial plane, have thereby lost much of their intensity; and the differences between the intensities at different points could only be detected by a resonator with more delicate adjustments. The cone of rays between the

second lens and its focal line is of almost uniform intensity in the neighbourhood of the axial plane. At the edge of the cone the intensity falls off very rapidly; and if the resonator be moved parallel to itself in a plane perpendicular to the axial plane, it shows sharply, by the commencement and stoppage of its sparking, where the boundaries of the cone lie. The cone is a little unsymmetrical on account of disturbance at the side nearest the wall, but the convergence of the rays to a focus is placed beyond a doubt.

The following observations were made on the rays after they had passed the focus of the second lens. The resonator, after having been set to spark at a distance of 80 centim. from the oscillator in the line of reference, was taken to the focus of the second lens, and there gave sparks of fair intensity. Beyond the focus there were traces of a divergence of the cone of rays, which became more evident when the oscillator knobs were quite clean; but in order to make sure of the existence of this divergence a more sensitive resonator would be necessary. In the axial plane itself the resonator used by us gave an effect when the conditions were most favourable, at a distance of 120 centim. beyond the focus of the second lens; and it would possibly have given an effect still further away, had there not been an iron hot-water pipe 9 centim. in diameter running from floor to ceiling of the passage near this point. The furthest point at which any traces of sparking could be found was in one case 450 centim. from the oscillator, while without the lenses it was only 120 centim. This statement has to be taken along with the fact that the lenses were only 180 centim. apart, and that no attempt was made to elongate the parallel portion of the beam by increasing their distance.

In order to determine experimentally the wave-length of the oscillations, a sheet of tin-plate was set up against the flat (inner) surface of the second lens. The rays reflected from this plate were thus made to interfere with those incident on it so as to give stationary waves, as in some experiments of Hertz. The result was that close to the plate there were no traces of sparking. On taking the resonator further away, however, the sparks appeared, reached a maximum, and then disappeared again at a distance of 50 centim. from the plate. The point of disappearance was very definite. The sparks appeared again when the resonator was still further withdrawn, and as long as it was kept parallel to the oscillator no further disappearance of the sparks could be observed. By rotating it, however, in the axial plane, a

point was found at which the amount of rotation required to make the sparks disappear was a minimum. In this position the centre of the resonator was 101 centim. from the reflecting plate. The observations agree with the previously calculated value of the wave-length, viz. 100 centim.

In the above experiments the oscillator was always placed in the focal line of the first lens, that is, vertically. Some observations were made later, after turning the oscillator through an angle so as to leave its centre in the axial plane, but its direction inclined to this plane. The effects were always of the same nature as those already described, even when both oscillator and receiver had been turned through a right angle, but the intensity of the radiation was not so great beyond the first lens. The focussing of the rays by the second lens could not be observed in this case, even when they were rotated only ten or twenty degrees, as the intensity was too small.

The above results all go to confirm the identity of electrical radiation and light; and are merely a slight extension of the famous researches of Hertz.

University College, Liverpool,
May 1889.

ADDENDUM dated June 20.

An expression for the self-induction of a straight copper rod, of length l and thickness d , we do not see how to calculate on Maxwellian principles without some sort of a return circuit somewhere. On action-at-a-distance principles it can be done thus:—

Consider two parallel filaments or thin straight wires at a distance c apart; call an element of one, at a distance a from some plane of reference, da , and an element of the other, at a distance b from the same plane, db . The mutual induction or potential of two elements on each other is

$$\frac{da db \cos \epsilon}{r},$$

where ϵ is the angle, and r the distance, between them. Hence the mutual induction of the two parallel filaments, each of length l , is

$$M = \int_0^l \int_0^l \frac{da db}{\sqrt{\{(a-b)^2 + c^2\}}}.$$

Integrating with regard to a , this becomes

$$M = \int_0^l \log \frac{\sqrt{\{(l-b)^2 + c^2\}} + (l-b)}{\sqrt{(b^2 + c^2)} - b} \cdot db,$$

where numerator and denominator are of the same form if the limits for the numerator portion have l subtracted from them both. Performing the integration of the two parts separately, and simplifying, we get

$$M = 2l \log \frac{\sqrt{(l^2 + c^2)} + l}{c} - 2 \{ \sqrt{(l^2 + c^2)} - c \}.$$

It may be worth while to write down the form this assume when c is moderately small compared with l , viz.

$$2l \left\{ \log \left(\frac{2l}{c} + \frac{c}{2l} \right) - \left(1 - \frac{c}{l} + \frac{c^2}{2l^2} \right) \right\}.$$

If we now put for c the geometric mean distance of the points in a cross section of a rod of thickness d , we shall have the mutual induction of the parts of all the filaments in that rod upon each other, *i. e.* the self-induction coefficient of the rod. And unless the rod is very short and thick, it will be permissible to neglect the c/l terms.

Now the geometric mean distance of the points in a circular section varies from $\frac{1}{2}d$, when they are concentrated into its circumference, to $\frac{1}{2}e^{-1}d$, or $\cdot 3894d$, when they are spread uniformly all over it. The first case corresponds to our rapidly periodic currents, and gives, as the self-induction of a rod in which currents keep to the periphery,

$$L = 2l \left(\log \frac{4l}{d} - 1 \right);$$

whereas if the currents penetrate all through its section, by reason of being of slowly changing strength,

$$L = 2l \left(\log \frac{4l}{d} - \frac{3}{4} \right).$$

The difference is not marked : at least for the case supposed, of non-magnetic material.

Hertz employed this last formula, quoting it apparently from Neumann ; but he says that in Maxwell's theory the $\frac{3}{4}$ turns into $\frac{1}{4}$. We do not know how he makes this out, but suppose he is somehow right ; and it is this uncertainty which has caused us to refrain from going into minutiae on the subject, and to be satisfied with using merely $\log \frac{4l}{d}$, instead of

$\left(\log \frac{4l}{d} - \text{something}\right)$, for what we have called the characteristic factor. It is easy to subtract 1 from it if that is the proper thing to do, as our calculation indicates it is. But the violent constriction at the spark, in the case of an oscillator, must cause a considerable increase of self-induction.

It may be interesting just to quote in similar form the self-induction of the same rod bent into a circle, viz.

$$2l \left(\log \frac{4l}{d} - 2 - \log \frac{\pi}{2} \right),$$

if the currents keep to its periphery. When they penetrate its section uniformly the 4 becomes 5.14, and that is all the change unless it is made of magnetic material.

It thus approaches the same value as the straight rod for infinite length, but is always distinctly less.

There is one point on which we find ourselves differing from Hertz. We regret to say that our calculation of radiation-intensity comes out four times as great as his. We get the same formula as he does, so there is no slip in the working there; but, in the application, a 2 or a $\sqrt{2}$ comes in wrongly in one or other of our calculations. His using half-wave lengths is a natural source of confusion, but we have avoided all that; and it must be that it is owing to a different calculation of the effective capacity concerned in an oscillator that the discrepancy arises. If an oscillator has spheres 30 centim. diameter at either end, Hertz calls its capacity 15 centim.; we call it $7\frac{1}{2}$. We feel bound to call it $7\frac{1}{2}$ according to any method of calculation; although the radius of either sphere is the natural thing to write down at first thought. The charge which surges into either sphere has had to come from the other, not from the earth or anything of infinite capacity. The two spheres are therefore like two condensers in series. Hence our wave-lengths are $1/\sqrt{2}$ of Hertz's wave-lengths (or rather $\sqrt{2}$ times what he calls his wave-length); and since λ occurs to the fourth power in radiation intensity, it makes our radiation 4 times as strong for a given oscillator as that which he would calculate. This discrepancy we by no means view lightly, and it is not without many qualms that we find ourselves differing, even about a 2, with a man so splendidly careful in his work as Hertz has shown himself. It is more than probable that he is right after all, so we explain what will then turn out to be our error in this note.

VIII. *Notices respecting New Books.*

Stellar Evolution and its Relations to Geological Time. By JAMES CROLL, LL.D., F.R.S., author of '*Climate and Time*,' &c. London: Stanford.

IN this little volume Dr. Croll continues and expands his now classic researches into the relations of Time and Geological Evolution. His investigations and speculations he now boldly carries to the utmost boundaries of time—if time is finite, and to the morning of creation, not only of the Earth and the solar system, but of the entire stellar universe. Beyond the point to which Dr. Croll ventures by his scientific imagination to pierce, Science certainly is not entitled to travel; but there is little doubt that long ere opinion finally settles itself into fixed belief as to that remote point and the cycle of events by which the starry hosts have come to be what they now appear, there will be many speculations to be hazarded, and many suggestions offered. Meantime we can only say that Dr. Croll has made a brave plunge into the unexplored; and if he has not finally settled the theory of creation, he has at least made a most substantial contribution towards the discussion of the great problem in physics which yet remains for philosophers of the foremost rank to settle.

The germ of the theory expounded in '*Stellar Evolution*' appeared in the pages of the *Philosophical Magazine* so long ago as May 1868. It was further expanded in '*Climate and Time*,' and in the more recent work '*Climate and Cosmology*.' Through an inquiry into the possible origin and age of the sun's heat, Dr. Croll is led to adopt and support the theory that the whole visible universe is the result of the collision of vast dark masses which have travelled through limitless space at various velocities and in independent paths. Thus with matter and motion in their most elementary condition the phenomena of creation began; and the progressive series of changes which we call Evolution only came into play when in boundless time and space two of these mighty dark masses clashed together, and by the partial or complete stoppage of their motion begat that energy of condition which manifested itself by the expansion of the solid masses into a gaseous nebula of enormous extent, heat, tenuity, and, from dissociation, of uniform chemical character. A nebula so created possesses a store of heat measured by the mass of the colliding bodies and the rate at which they were travelling at the period of collision. There is indeed no necessary limit to the store of energy which might in this way be vested in a nebulous mass. Dealing with gravitational energy alone, on the other hand, the amount available in any system is strictly limited. It has been shown by Helmholtz and Sir William Thomson that the solar system cannot be older than from twelve to twenty millions of years if its heat is due to gravitation alone. That amount of time Dr. Croll goes on to show is utterly inadequate for the evolution of terrestrial phenomena, and a considerable portion of his work is occupied in marshalling

a striking series of geological facts which demonstrate the much greater age of the solar system. A quarter of a century ago Dr. Croll first pointed out the important evidence afforded by sub-aerial denudation as to the antiquity of the Earth. By that scale and by other concurrent sources of testimony he concludes that the Earth must have existed in a condition not greatly different from what now prevails for at least seventy millions of years. That being so, we are bound to seek a source of vastly greater heat than can be derived from simple shrinkage of a nebula. Such a source Dr. Croll finds in his "Impact" theory of solar genesis, and he supports his theory by many ingenious arguments. Sir William Thomson regards it as enormously less probable than the gravitation theory, on account of the necessary assumption of exact aiming of the colliding bodies; but the probability of the collision of dark masses is a question of their numbers, distribution, and of time; and against the gravitation theory there is the fact that the actual motion observable in many stars cannot have been derived from that source. Further, it may be said that the "Impact" theory appears to get remarkable support from the recent important researches of Dr. Huggins regarding the constitution of nebulae, the results of which he communicated to the Royal Society during the past session.

Cosmic Evolution, being Speculations on the Origin of our Environment. By E. A. RIDSDALE. London: Lewis.

MR. RIDSDALE, in his essay, assumes that the universe in its primordial condition consisted of a uniform gaseous expansion possessed of an inconceivably high temperature. Chemical combination became possible only as temperature of this attenuated matter decreased; and with each successive combination there was, in accordance with well-known laws, a shrinkage in volume. Chemical activity was at first violently energetic; but as inorganic evolution proceeded, elements differentiated and compounds increased, more stable conditions arose, and the chemically inert survived to form a basis favourable for the production and maintenance of life and organic compounds. Mr. Ridsdale develops his thesis in a rather inconsequent manner; and although in their general bearings his speculations may be accepted as satisfactory, he trenches on subjects which are too profound and vast to be fairly within the grasp of his limited knowledge and experience.

IX. *Proceedings of Learned Societies.*

GEOLOGICAL SOCIETY.

[Continued from vol. xxvii. p. 435.]

April 17, 1889.—W. T. Blanford, LL.D., F.R.S., President,
in the Chair.

THE following communications were read:—

1. "On the Production of Secondary Minerals at Shear-zones in the Crystalline Rocks of the Malvern Hills." By Charles Callaway, Esq., M.A., D.Sc., F.G.S.

In a previous communication the author had contended that

many of the schists of the Malvern Hills were of igneous origin. Thus, mica-gneiss had been formed from granite, hornblende-gneiss from diorite, mica-schist from felsite, and injection-schists from veined complexes which had been subjected to compression. As a further instalment towards the elucidation of the genesis of the Malvern schists, it was now proposed to discuss the changes which the respective minerals of the massive rocks had undergone in the process of schist-making.

The schistosity was usually in zones, striking obliquely across the ridge, varying in breadth from a few inches to many yards, and separated from each other by very irregular intervals. Within the zones bands of maximum schistosity alternated with seams in which the original structure had been less completely obliterated. The new structure was connected with a shearing movement, by which the rigid mass was often sliced into countless parallel laminae or flakes. In a more advanced stage of alteration, the planes of movement were obliterated, and a sound clear gneiss or schist was formed. These foliated bands were called "shear-zones."

The most important shear-zones were those in which diorite was interlaced with granite-veins. The following changes were noticed in tracing the massive rocks into the zones. The hornblende might suffer excessive corrosion, or it might become "reedy" and break up along the cleavages into numerous fragments, which were drawn away from each other in the direction of foliation, or it might pass into chlorite, or chlorite and epidote. The chlorite thus formed often passes into biotite, and sometimes the biotite was changed to white mica. Where shearing was excessive, chlorite sometimes passed directly into white mica.

Soda-lime felspar was altered to epidote or zoisite, and often to calcite. A more important result was the production of muscovite in the plagioclase. Much of this felspar was reconstructed in small clear crystals or granules. Quartz also was abundantly produced. Diorite might thus be converted either into a gneiss with two micas, or into a gneissoid quartzite. The granite of the veins passed through the usual changes into muscovite-gneiss.

Other secondary minerals were actinolite (from augite), sphene (from ilmenite), and garnet.

It was contended that the granite-veins were exogenous, because they appeared as apophyses from large masses; they had the same coarse texture in different varieties of diorite, and they produced contact-effects similar to those of intrusive veins, including the phenomena of aggregation and enlargement in the minerals of the encasing rock.

Foreign minerals were often introduced by infiltration. Thus, the hornblende of a diorite was decomposed into chlorite and iron-oxide, which passed for a considerable distance along the shear-planes of an adjacent granite, giving rise to a chlorite-gneiss, and the chlorite was partially changed to biotite. Epidote might be introduced in the same way.

Both the diorite and the granite of shear-zones tended by loss of bases to become progressively silicified. Most of the liberated bases

could be accounted for. Analyses showed that there was an interchange of alkaline bases, soda going to the granite, and potash to the diorite. Thus, some of the latter contained almost twice as much potash as soda.

The evidence collected seemed to prove that the schist-making had taken place subsequently to consolidation; but it was clear, especially where the rock was heavily sheared, that the constituents had been redissolved and reconstructed. Thus, as we followed a diorite into the core of a shear-zone, we could see the gradual disappearance of shear-planes and other mechanical effects, as well as the progressive results of chemical synthesis.

The secondary origin of the micas and of part of the felspar was proved by the fact that they were moulded on decomposition-products, such as chlorite and epidote, and upon fragments of hornblende crystals, which had been crushed during the shearing, and carried away from each other. The mineral changes here described resulted from contact-action *plus* mechanical force.

2. "The Northern Slopes of Cader Idris." By Grenville A. J. Cole, Esq., F.G.S., and A. V. Jennings, Esq., F.L.S.

From the publication of Mr. Aikin's paper in the Transactions of the Geological Society in 1829 to the second edition of the Survey Memoir on North Wales, the relations of the geological and physical features of Cader Idris have been pointed out in some detail. The present paper dealt with the nature of the eruptions that took place in this area and the characters of their products at successive stratigraphical horizons. The best exposures occur, as is well known, upon the northern slopes.

The lowest evidence of contemporaneous volcanic activity is to be found at the Penrhyn-gwyn slate-quarry, where a somewhat coarse bed of tuff, with slate-fragments and abundant felspar-crystals, occurs above an andesitic sheet. Similar slate-tuffs are repeated up to the base of the great cliff of Cader Idris, with intervening layers of normal clayey sediment. On the whole, the tuffs and ashes become more highly silicated as the upper levels are reached, and they terminate on the southern slopes in beds with fragments of perlite and devitrified obsidian, such as are found under Craig-y-Llam. On Mynydd-y-Gader the intrusive dolerites have altered the ashes into hornstones; in places, moreover, they have become jointed into distinct columns. Fragments of andesitic glass as well as trachyte are recorded.

The "pisolitic iron-ore" of the Arenig beds appears to have resulted from the metamorphism of an oolitic limestone, as in the case of the Cleveland ore described by Mr. Sorby, and that of Northampton described by Prof. Judd. The grains still give evidence under crossed nicols of their having been built up of successively deposited concentric layers. The calcite so freely developed in the hollows of the underlying rocks may have been largely derived, during metamorphic action, from the destruction of similar thin limestone-seams. No true lava-flows occur among

these tuffs and sediments, a fact that implies comparative remoteness from the volcanic centre; and the important masses of intrusive matter represented upon the maps are themselves largely composed of the products of explosive action. The numerous sheets of ophitic dolerite, aphanite, and altered andesite, that lie, seemingly interbedded, on the northern slopes, were probably intruded when the associated rocks were already weighed down by much superincumbent sediment. A common character of these basic sheets is the development of small colourless crystals of epidote.

The most striking mass upon the mountain is the main "felstone" (eurite) of the wall, which proves to be minutely "granophyric," and of very uniform grain throughout. An analysis by Mr. T. H. Holland shows 73 per cent. of silica. This vast intrusive sheet is regarded as perhaps of no later date than the Llandeilo lavas of Craig-y-Llam, and as a forerunner of the volcanic conditions that prevailed in Bala times throughout North Wales.

The stratigraphical horizons, as shown on published sections, would throw a great part of the tuffs and ashes described into the Tremadoc beds, or even lower, in contradiction to the generally accepted statement that volcanic activity began in the Arenig times. While this point can only be settled by detailed mapping on the basis of the new six-inch survey, the authors incline to the belief that the eruptions in this area broke out in the Cambrian rather than the Ordovician period.

May 8.—W. T. Blanford, LL.D., F.R.S., President,
in the Chair.

The following communications were read:—

1. "The Rocks of Alderney and the Casquets." By the Rev. Edwin Hill, M.A., F.G.S.

The author in this paper described Alderney, Burhou, with its surrounding reefs, and the remoter cluster of the Casquets, all included within an area about 10 miles long.

Alderney itself consists in most part of crystalline igneous rocks, hornblendic granites of varying constitution which resemble some Guernsey rocks, but seem more nearly connected with those of Herm and Sark. These are pierced by various dykes, and among them by an intrusion containing olivine, which may be placed with the group of picrites. There is also in the island a dyke of mica-trap.

The eastern part only of Alderney, but the whole of Burhou, the Casquets and their neighbouring reefs, consist of stratified rocks. These contain rare beds of fine mudstone, but are generally false-bedded sandstones, and grits, sometimes with pebbles, often rather coarse and angular, occasionally becoming typical arkoses. At a point on the southern cliffs of Alderney they may be seen to rest on the crystalline igneous mass. A series identical in constitution and aspect occurs at Omonville, on the mainland, a few miles east of Cap La Hague (as had also been noticed a few months earlier by

M. Bigot). These have been correlated with others near Cherbourg, and described as underlying the "grès Armoricaïn." The Alderney grits, therefore, form part of a series which can be traced over 30 miles, and which belongs to the Upper Cambrian (of Lapworth).

Remarks were made on the Jersey conglomerates (Ansted's conjectural identification of these with the Alderney grits being approved), on the resulting evidence that the Jersey rhyolites are not Permian, but Cambrian at the latest, on the still earlier age of the Guernsey syenites and diorites, and on the antiquity of the Guernsey gneisses.

2. "On the Ashprington Volcanic Series of South Devon." By the late Arthur Champernowne, Esq., M.A., F.G.S.

The author described the general characters of the volcanic rocks that occupy a considerable area of the country around Ashprington, near Totnes. They comprise tuffs and lavas, the latter being sometimes amygdaloidal and sometimes flaggy and aphanitic. The aphanitic rocks approach in character the porphyritic "schalsteins" of Nassau. Some of the rocks are much altered; the felspars are blurred, as if changing to saussurite, like the felspars in the Lizard gabbros. In other cases greenish aphanitic rocks have, by the decomposition of magnetite or ilmenite, become raddled and earthy in appearance, so as to resemble tuffs. The beds are clearly intercalated in the Devonian group of rocks, and the term Ashprington Series is applied to them by the author. Although this series probably contains some detrital beds, there are no true grits in it. Stratigraphically the series appears to come between the Great Devon Limestone and the Cockington Beds, the evidence not being discussed by the author, however, so fully as he had intended, as the paper was not completed.

X. Intelligence and Miscellaneous Articles.

ON A POSSIBLE GEOLOGICAL ORIGIN OF TERRESTRIAL MAGNETISM. BY PROFESSOR EDWARD HULL, M.A., LL.D., F.R.S., DIRECTOR OF THE GEOLOGICAL SURVEY OF IRELAND.*

THE author commenced by pointing out that the origin and cause of terrestrial magnetism were still subjects of controversy amongst physicists; and this paper was intended to show that the earth itself contains within its crust a source to which these phenomena may be traced, as hinted at by Gilbert, Biot, and others; though, owing to the want of evidence regarding the physical structure of our globe in the time of these observers, they were unable to identify the earth's supposed internal magnet.

The author then proceeded to show cause for believing that there exists beneath the crust an outer and inner envelope or

* Communicated by the Author, being an Abstract of a paper communicated to the Royal Society, 16 May, 1889.

"magma"—the former less dense and highly silicated, the latter basic and rich in magnetic iron-ore. This view was in accordance with those of Durocher, Prestwich, Fisher, and many other geologists. The composition of this inner magma, and the condition in which the magnetic iron-ore exists were then discussed, and it was shown that it probably exists under the form of numerous small crystals with a polar arrangement. Each little crystal being itself a magnet and having crystallized out from the magma while this latter was in a viscous condition, the crystalline grains would necessarily assume a polar arrangement which would be one of equilibrium. Basalt might be taken as the typical rock of this magma.

The thickness and depth of the magnetic magma beneath the surface of the globe were then discussed, and while admitting that it was impossible to come to any close determination on these points owing to our ignorance of the relative effects of increasing temperature and pressure, it was assumed tentatively that the outer surface of the effective magnetic magma might be at an average depth of about 100 miles, and the thickness about 25 or 30 miles. The proportion of magnetic iron-ore in basaltic rocks was then considered, and it was shown that an average of 10 to 15 per cent. would express these proportions; and assuming similar proportions to exist in the earth's magnetic magma, we should then have an effective terrestrial magnet of from $2\frac{1}{2}$ to 3 miles in thickness. The thickness is, however, probably much greater.

Instances of polarity in basaltic masses at various localities were adduced in order to illustrate the possibility of polarity in the internal mass.

The subject of the polarity of the globe was then discussed, and it was pointed out how the position of the so-called "magnetic poles" leads to the inference that they are in some way dependent upon the position of the terrestrial poles.

The author regarded the double so-called "poles" as merely foci due to protuberances of the magnetic magma into the exterior non-magnetic magma, and that there was really only a single magnetic pole in each hemisphere, embracing the whole region round the terrestrial pole and the *stronger and weaker magnetic foci*, and roughly included within the latitude of 70° within the northern hemisphere.

It was pointed out that the poles of a bar-magnet embrace a comparatively large area of its surface, and hence a natural terrestrial magnet of the size here hypothecated may be inferred to embrace a proportionably large tract for its poles.

In reference to the question why the magnetic poles are situated near those of the earth itself, this phenomenon seemed to be connected with the original consolidation of the crust of the globe, and the formation of its internal magmas.

It was pointed out that, in the case of the magnetic magma the process of crystallization and the polar arrangement of the particles of magnetic iron-ore would proceed in a radial direction. The

manner in which the phenomena of magnetic intensity, and of the dip of the needle at different latitudes could be explained on the hypothesis of an earth's internal magnetic shell, such as here described, was then pointed out; and the analogy of such a magnetic shell with a magnetic bar passing through the centre of the earth was illustrated.

The author then proceeded to account on geo-dynamical principles for the secular variation of the magnetic needle, and also to show how the objections that might be raised to the views here advanced, on the grounds of the high temperature which must be assumed to exist at the depth beneath the surface of the magnetic magma, could be met by considerations of pressure, and on this subject read a letter which he had received from Sir William Thomson, F.R.S.

In conclusion, the author stated it was impossible in a short abstract to go into the details of the subjects here discussed, and for further information the reader must be referred to the paper itself.

NOTES ON METALLIC SPECTRA. BY C. C. HUTCHINS.

In the work herein described an attempt has been made to determine the wave-length of several metallic lines with something of the precision with which wave-lengths of solar lines are known and tabulated.

It has been repeatedly pointed out that wave-lengths of metallic lines from the determinations of the best observers are liable to errors of one part in 3000 or 4000; while Rowland has given us the position of a long list of solar lines correct to one part in 500,000. It is too often forgotten that Thalén used a single bisulphide-of-carbon prism in his researches, and that consequently his places can in no sense be considered standards of precision for the more powerful instruments of the present time.

The spectroscope employed in the present work has a large flat grating with ruled space 5 by 8 centim. Upon the margin of this grating Professor Rowland has written: "Definitions exquisite." The collimator and view-telescope are combined in a single lens, an excellent objective by Wray, 6 inches in diameter, $8\frac{1}{2}$ feet focus. The radius of curvature of the back surface of this lens equals its focal length, so that the ray reflected from this surface passes back to the slit, and any objectionable illumination of the field is avoided. All parts of the instrument are so contrived that it is operated without the necessity of the observer leaving his seat at the eyepiece. A heliostat and achromatic lens of 5-feet focus form an image of the sun upon the slit. Thus arranged, the instrument easily performs all tests of spectroscopic excellence with which the writer is familiar. To produce the metallic spectra an 8-inch spark, condensed by a number of jars having about six square feet of coated surface, has been employed. The spark is produced immediately before the slit, the jaws of which open equally. The coil is operated sometimes by a dynamo, and sometimes by the

current from a storage-battery. A review of all the spark spectrum-lines has been made with the arc, and a few lines added to those that the spark gave. A steam-jet* was employed to increase the luminosity of the spark. The work has been confined to the lower portion of the spectrum, where it still appears that eye-observations have advantages of the photographs.

The position of a metallic line is determined by bringing the crosswire of the micrometer upon it, letting in the sunlight, and moving the crosswire to one of the standard lines of Rowland's tables. The true wave-length of the metallic line can then be computed from a previously determined micrometer-constant. As a check to the result so obtained the metallic line has been interpolated, with the micrometer, between two of the standard lines in the same field of view, and the whole process has been repeated on different days until it became assured that positions of the metallic lines were as precise as those of the standard lines themselves.

Copper Spectrum.

The subjoined table gives the results as obtained for the spectrum of copper. The first two columns contain respectively the wave-lengths and intensities as given by Thalén; the third, the wave-lengths as determined in the present work.

Copper Spectrum.

Thalén λ .		Corrected λ .	Remarks.
6379·7	2	6380·899	Surrounded by continuous spectrum. Reversed in sun.
6218·3	5	No line seen.
5781·3	2	5782·285	Reversed in arc. Reversed in sun.
5700·4	1	5700·442	Reversed in arc. Reversed in sun.
		5535·64	Reversed in sun. Seen only in the arc.
		5555·119	Seen only in the arc.
5292·0	2	5292·68	Reversed in sun.
5217·1	1	5218·308	Reversed in arc. Reversed in sun.
5152·6	1	5153·345	Reversed in arc. Reversed in sun.
5104·9	1	5105·663	Reversed in sun.
5011·4	4	No line seen.
		5016·86	Seen only in the arc.
4955·5	3	No line seen.
4932·5	3	4933·181	Reversed in sun.

Zinc Spectrum.

The examination of the zinc spectrum is here limited to five lines; many of the remaining lines being mere dots close to the poles of the metal, others very broad and nebulous, and in general too ill defined to admit of measurement with the apparatus employed. In strong contrast to the remaining lines these five are very bright and sharp, and may be called the representative lines of the metal within those limits.

* Silliman's American Journal, Feb. 1889; Phil. Mag. Feb. 1889.

Zinc Spectrum.

Thalén λ .		Corrected λ .	Remarks.
6362.5	1	6362.566	Reversed in sun.
		6204.708	Faint but very sharp. Reversed in sun.
5893.5	2	5894.454	
4809.7	1	4810.671	Very bright. Reversed in sun.
4721.4	1	4722.306	Very bright. Reversed in sun.

Results of comparison with Solar Spectrum.—It will be seen by inspection of the tabulated results that nine out of the eleven lines of copper are reversed in the sun, and four out of the five of zinc. The conclusion reached in each of these cases was after repeated examination, when the conditions were such as to show a clear space between the components of the E line. The latest available authority* gives copper among the doubtful elements in a list of those found in the sun, and on the same list zinc does not appear at all. The present investigation makes it quite probable that zinc, and almost completely demonstrates that copper, exists in the solar atmosphere.—Silliman's *American Journal of Science*, June 1889.

ON THE INFLUENCE OF SOLAR RADIATION ON THE ELECTRICAL PHENOMENA IN THE ATMOSPHERE OF THE EARTH. BY SV. ARRHENIUS.

In earlier researches (Wied. *Ann.* vol. xxxii. p. 546, and xxxiii. p. 638) the author has concluded from a series of investigations that the air, when irradiated by ultra-violet light, conducts like an electrolyte. Starting from Peltier's hypothesis of a negative charge of the earth, the author makes use of this point of view to represent the electrical phenomena of the atmosphere as consequences of solar radiation. The earth's charge, according to the author, is neither imparted to the molecules of the air as shown by the experiments of Nahrwold, nor does the aqueous vapour ascending from the earth carry electricity with it, for which the experiments of Kalischer, Magrini, and Blake speak. The carriers of the electricity in the atmosphere are the solid and liquid particles suspended in it (dust, fog-vesicles); and they obtain their charge from the earth by conduction, when the air becomes a conductor in consequence of the influence of the sun's rays. There is then a very feeble electrical current in the air. The author considers that a proof of this is met with in the formation of ozone in the atmosphere, for which, according to Wurster, sunshine and liquid deposits are necessary.

It is in accordance with the assumption of a negative charge of the suspended particles, that on cloudy days the fall of potential is much lower than on bright ones; that deposits, especially hail, are for the most part negative, while snow is occasionally positive, because it occurs at the time at which the sun's action is weakest; in like manner he considers that the positive fall of potential observed

* Young's 'General Astronomy.'

in the morning fog is due to the sun not having as yet acted on the fog. The author adduces a table of Quetelet, which shows as the result of many years' observations that the monthly mean of the strength of atmospheric electricity is the less, the greater is the monthly mean of the solar radiations measured with the actinometer.

The author explains in the well-known manner the formation of the high tensions observed in storms. As the first condition in the original charge of the drops is the solar radiation, thunderstorms are in causal connexion with this, and are most frequent in hot countries in summer, and in the afternoon. That the maximum daily occurrence of thunderstorms is somewhat behind that of the solar radiation is ascribed by the author to the time required for the charge and for the coalescence of the individual drops. The other meteorological phenomena which accompany the storms are considered by the author to be secondary. The more infrequent whirlwind storms, which, in contradistinction to the heat-storms, occur mostly in winter, and at night, and which cannot be explained on the above principles, were supposed to be brought from more southern regions.

The author finds a further support for his theory in the course of the yearly and of the daily periods of atmospheric electricity. The yearly period, with its winter maximum and summer minimum, would directly correspond to the influence mentioned. The daily period is more complicated, for it exhibits two maxima and two minima. But this may also be explained, if to the influence of the solar radiation the influence be added which must be exerted by the ascending motion of the negative clouds at daytime and the descent at night-time. The author observes that Exner's theory, according to which aqueous vapour is the carrier of negative electricity, is quite in disaccordance with the facts of the daily period of atmospheric electricity.

In conclusion the author develops the result that, since the greater part of the active solar radiation is absorbed in the upper regions, these must conduct far better than the lower layers of air. Now since these upper layers of air are moving strongly from west to east round the earth, a conductor would be obtained under the influence of the sun's rays, which rotated about a magnet (the earth); by unipolar induction electromotive forces must be set up which would drive the electricity in the higher atmosphere from the equator to the pole. The aurora boreales and the daily variations of terrestrial magnetism would be referred to these phenomena. The author observes that Schuster and Balfour Stewart had already assumed an increase of the electrical conducting power of the air with the sun's rays; they, however, had sought for the cause in a heating of the layers of air, an hypothesis the improbability of which had already been demonstrated by Hann.—*Meteor. Zeitschrift*, vol. v. p. 297 (1888); *Beiblätter der Physik*, vol. xiii. p. 328.

THE
LONDON, EDINBURGH, AND DUBLIN
PHILOSOPHICAL MAGAZINE
AND
JOURNAL OF SCIENCE.

[FIFTH SERIES.]

AUGUST 1889.

XI. *On Achromatic Interference-Bands.* By Lord RAYLEIGH,
*Sec. R.S., Professor of Natural Philosophy in the Royal
Institution*.*

Introduction.

WHEN there is interference of light, the width of the resulting bands, measured for example from darkness to darkness, is usually a function of the colour of the light employed. Thus, in the case of Fresnel's well-known interference-experiment, in which light reflected from two slightly inclined mirrors illuminates a screen, the width of the bands is proportional to the wave-length of the light. In order that a considerable number of bands may be visible, it is necessary that the light be highly homogeneous; otherwise it is impossible that the various band-systems can fit one another over the necessary range. If the light could be supposed to be absolutely homogeneous, there would be no limit to the number of observable bands: and, what is especially to be remarked, there would be nothing by which one band could be distinguished from another,—in particular there could be no central band recognizable. When, on the other hand, the light is white, there may be a central band at which all the maxima of brightness coincide; and this band, being white, may be called the achromatic band. But the *system* of bands is not usually achromatic. Thus, in Fresnel's experiment the centre of symmetry fixes the position of the central achromatic band,

* Communicated by the Author.

but the system is far from achromatic. Theoretically there is not even a single place of darkness, for there is no point where there is complete discordance of phase for all kinds of light. In consequence, however, of the fact that the range of sensitiveness of the eye is limited to less than an "octave," the centre of the first dark band on either side is sensibly black; but the existence of even one band is due to selection, and the formation of several visible bands is favoured by the capability of the retina to make chromatic distinctions within the range of vision. After two or three alternations the bands become highly coloured*; and, as the overlapping of the various elementary systems increases, the colours fade away, and the field of view assumes a uniform appearance.

There are, however, cases where it is possible to have systems of achromatic bands. For this purpose it is necessary, not merely that the maxima of illumination should coincide at some one place, but *also* that the widths of the bands should be the same for the various colours. The independence of colour, as we shall see, may be absolute; but it will probably be more convenient not to limit the use of the term so closely. The focal length of the ordinary achromatic object-glass is not entirely independent of colour. A similar use of the term would justify us in calling a system of bands achromatic, when the width of the elementary systems is a maximum or a minimum, for some ray very near the middle of the spectrum, or, which comes to the same, has equal values for two rays of finitely different refrangibility. The outstanding deviation from complete achromatism, according to the same analogy, may be called the *secondary colour*.

The existence of achromatic systems was known to Newton†, and was insisted upon with special emphasis by Fox Talbot‡; but singularly little attention appears to have been bestowed upon the subject in recent times. In the article "Wave Theory" (*Encyc. Brit.* 1888) I have discussed a few cases, but with too great brevity. It may be of interest to resume the consideration of these remarkable phenomena, and to detail some observations which I have made, in part since the publication of the 'Encyclopædia' article. A recent paper by M. Mascart § will also be referred to.

* The series of colours thus arising are calculated, and exhibited in the form of a curve upon the colour diagram, in a paper "On the Colours of Thin Plates," Edinb. Trans. 1887.

† 'Optics,' Book ii.

‡ Phil. Mag. [3] ix. p. 401 (1836).

§ "On the Achromatism of Interference," *Comptes Rendus*, March 1889; Phil. Mag. [5] xxvii. p. 519.

Fresnel's Bands.

In this experiment the two sources of light which are regarded as interfering with one another must not be independent; otherwise there could be no fixed phase-relation. According to Fresnel's original arrangement the sources O_1 , O_2 are virtual images of a single source, obtained by reflexion in two mirrors. The mirrors may be replaced by a bi-prism. Or, as in Lloyd's form of the experiment, the second source may be obtained from the first by reflexion from a mirror placed at a high degree of obliquity. The screen upon which the bands are conceived to be thrown is parallel to O_1O_2 , at distance D . If A be the point of the screen equidistant from O_1 , O_2 , and P a neighbouring point, then approximately

$$O_1P - O_2P = \sqrt{\{D^2 + (u + \frac{1}{2}b)^2\}} - \sqrt{\{D^2 + (u - \frac{1}{2}b)^2\}} = ub/D,$$

where

$$O_1O_2 = b, \quad AP = u.$$

Thus, if λ be the wave-length, the places where the phases are accordant are determined by

$$u = n\lambda D/b, \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

n being an integer representing the order of the band. The linear width of the bands (from bright to bright, or from dark to dark) is thus

$$\Lambda = \lambda D/b. \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

The degree of homogeneity necessary for the approximate perfection of the n th band may be found at once from (1) and (2). For, if du be the change in u corresponding to the change $d\lambda$, then

$$du/\Lambda = n d\lambda/\lambda. \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

Now clearly du must be a small fraction of Λ , so that $d\lambda/\lambda$ must be many times smaller than $1/n$, if the darkest places are to be sensibly black. But the phenomenon will be tolerably well marked, if the proportional range of wave-length do not exceed $1/(2n)$, provided, that is, that the distribution of illumination over this range be not concentrated towards the extreme parts.

So far we have supposed the sources at O_1 , O_2 to be mathematically small. In practice the source is an elongated slit, whose direction requires to be carefully adjusted to parallelism with the reflecting surface, or surfaces. By this means an important advantage is obtained in respect of brightness without loss of definition, as the various parts of the aperture give rise to coincident systems of bands.

The question of the admissible *width* of the slit requires

careful consideration. We will suppose in the first place that the lights issuing from the various parts of the aperture are without permanent phase-relation, as when the slit is backed immediately by a flame, or by the incandescent carbon of an electric lamp. Regular interference can then only take place between lights coming from *corresponding* parts of the two images; and a distinction must be drawn between the two ways in which the images may be situated relatively to one another. In Fresnel's experiment, whether carried out with mirrors or with bi-prism, the corresponding parts of the images are on the same side; that is, the right of one corresponds to the right of the other, and the left of one to the left of the other. On the other hand, in Lloyd's arrangement the reflected image is reversed relatively to the original source: the two outer edges corresponding, as also the two inner. Thus, in the first arrangement the bands due to various parts of the slit differ merely by a lateral shift, and the condition of distinctness is simply that the width of the slit be a small proportion of the width of the bands. From this it follows as a corollary that the limiting width is independent of the order of the bands under examination. It is otherwise in Lloyd's method. In this case the centres of the systems of bands are the same, whatever part of the slit be supposed to be operative, and it is the distance apart of the images (b) that varies. The bands corresponding to the various parts of the slit are thus upon different scales, and the resulting confusion must increase with the order of the bands. From (1) the corresponding changes in u and b are given by

$$du = -n\lambda D \, db/b^2;$$

so that

$$du/\Lambda = -n \, db/b \quad . \quad . \quad . \quad . \quad . \quad (4)$$

If db represents twice the width of the slit, (4) gives a measure of the resulting confusion in the bands. The important point is that the slit must be made narrower as n increases, if the bands are to retain the same degree of distinctness.

If the various parts of the width of the slit do not act as independent sources of light, a different treatment would be required. To illustrate the extreme case, we may suppose that the waves issuing from the various elements of the width are all in the same phase, as if the ultimate source were a star situated a long distance behind. It would then be a matter of indifference whether the images of the slit, acting as proximate sources of interfering light, were reversed relatively to one another, or not. It is, however, unnecessary to dwell upon this question, inasmuch as the conditions supposed are

unfavourable to brightness, and therefore to be avoided in practice. The better to understand this, let us suppose that the slit is backed by the sun, and is so narrow that, in spite of the sun's angular magnitude, the luminous vibration is sensibly the same at all parts of the width. For this purpose the width must not exceed $\frac{1}{20}$ millim.* By hypothesis, the appearance presented to an eye close to the slit and looking backwards towards the sun will be the same as if the source of light were reduced to a point coincident with the sun's centre. The meaning of this is that, on account of the narrowness of the aperture, a point would appear dilated by diffraction until its apparent diameter became a large multiple of that of the sun. Now it is evident that in such a case the brightness may be enhanced by increasing the sun's apparent diameter, as can always be done by optical appliances. Or, which would probably be more convenient in practice, we may obtain an equivalent result by so designing the experiment that the slit does not require to be narrowed to the point at which the sun's image begins to be sensibly dilated by diffraction. The available brightness is then at its limit, and would be no greater, even were the solar diameter increased. The practical rule is that, when brightness is an object, slits backed by the sun should not be narrowed to much less than half a millimetre.

Lloyd's Bands.

Lloyd's experiment deserves to be more generally known, as it may be performed with great facility and without special apparatus. Sunlight is admitted horizontally into a darkened room through a slit situated in the window-shutter, and at a distance of 15 or 20 feet is received at nearly grazing incidence upon a vertical slab of plate glass. The length of the slab in the direction of the light should not be less than 2 or 3 inches, and for some special observations may advantageously be much increased. The bands are observed on a plane through the hinder vertical edge of the slab by means of a hand magnifying-glass of from 1 to 2 inch focus. The obliquity of the reflector is of course to be adjusted according to the fineness of the bands required.

From the manner of their formation it might appear that under no circumstances could more than half the system be visible. But, according to Airy's principle†, the bands may be displaced if examined through a prism. In practice all

* Verdet's *Leçons d'Optique physique*, t. i. p. 106.

† See below.

that is necessary is to hold the magnifyer somewhat excentrically. The bands may then be observed gradually to detach themselves from the mirror, until at last the complete system is seen, as in Fresnel's form of the experiment.

If we wish to observe interference under high relative retardation, we must either limit the light passing the first slit to be approximately homogeneous, or (after Fizeau and Foucault) transmit a narrow width of the band-system itself through a second slit, and subsequently analyse the light into a spectrum. In the latter arrangement, which is usually the more convenient when the original light is white, the bands seen are of a rather artificial kind. If, apart from the heterogeneity of the light, the original bands are well formed, and if the second slit be narrow enough, the spectrum will be marked out into bands; the bright places corresponding to the kinds of light for which the original bands would be bright, and the black places to the kinds of light for which the original bands would be black. The condition limiting the width of the second slit is obviously that it be but a moderate fraction of the width of a band (Δ).

If it be desired to pass along the entire series of bands up to those of a high order by merely traversing the second slit in a direction perpendicular to that of the light, a very long mirror is necessary. But when the second slit is in the region of the bands of highest order (that is, near the external limit of the field illuminated by both pencils), only the more distant part of the mirror is really operative; and thus, even though the mirror be small, bands of high order may be observed, if the second slit be carried backwards, keeping it of course all the time in the narrow doubly-illuminated field. In one experiment the distance from the first slit to the (3-inch) reflector was 27 feet, while the second slit was situated behind at a further distance of 4 feet. The distance (b) between the first slit and its image in the reflector (measured at the window) was about 13 inches.

As regards the spectroscope it was found convenient to use an arrangement with detached parts. The slit and collimating lens were rigidly connected, and stood upon a long and rigid box, which carried also the mirror. The narrowness of the bands in which this slit is placed renders it imperative to avoid the slightest relative unsteadiness or vibration of these parts. The prisms, equivalent to about four of 60° , and the observing telescope were upon another stand at a little distance behind the box which supported the rest of the apparatus.

Under these conditions it was easy to observe bands in the spectrum whose width (from dark to dark) could be made as

small as the interval between the D lines ; but for this purpose the first slit had to be rather narrow, and the direction of its length accurately adjusted, so as to give the greatest distinctness. Since the wave-lengths of the two D lines differ by about $\frac{1}{1000}$ part, spectral bands of this degree of closeness imply interference with a retardation of 1000 periods.

Much further than this it was not easy to go. When the bands were rather more than twice as close, the necessary narrowing of the slits began to entail a failing of the light, indicating that further progress would be attained with difficulty.

Indeed, the finiteness of the illumination behind the first slit imposes of necessity a somewhat sudden limit to the observable retardation. In this respect it is a matter of indifference at what angle the reflector be placed. If the angle be made small, so that the reflexion is very nearly grazing, the bands are upon a larger scale, and the width of the second slit may be increased, but in a proportional degree the width of the first slit must be reduced.

The relation of the width of the second slit to the angle of the mirror may be conveniently expressed in terms of the appearance presented to an eye placed close behind the former. The smallest angular distance which the slit, considered as an aperture, can resolve, is expressed by the ratio of the wave-length of light (λ) to the width (w_2) of the slit. Now, in order that this slit may perform its part tolerably well, w_2 must be less than $\frac{1}{2}\lambda$; so that, by (2),

$$\lambda/w_2 > 2b/D. \quad . \quad . \quad . \quad . \quad . \quad (5)$$

The width must therefore be less than the half of that which would just allow the resolution of the two images (subtending the angle b/D) as seen by an eye behind. In setting up the apparatus this property may be turned to account as a test.

The existence of a limit to n , dependent upon the intrinsic brightness of the sun, may be placed in a clearer light by a rough estimate of the illumination in the resulting spectrum ; and such an estimate is the more interesting on account of the large part here played by diffraction. In most calculations of brightness it is tacitly assumed that the ordinary rules of geometrical optics are obeyed.

Limit to Illumination.

The narrowness of the second slit would not in itself be an obstacle to the attainment of full spectrum brightness, were we at liberty to make what arrangements we pleased behind

it. In illustration of this, two extreme cases may be considered of a slit illuminated by ordinary sunshine. First, let the width w_2 be great enough not sensibly to dilate the solar image; that is, let w_2 be much greater than λ/s , where s denotes in circular measure the sun's apparent diameter (about 30 minutes). In this case the light streams through the slit according to the ordinary law of shadows, and the pupil (of diameter p) will be filled with light if situated at a distance exceeding d^* , where

$$p/d = s. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (6)$$

At this distance the apparent width of the slit is w_2/d , or $w_2 s/p$; and the question arises whether it lies above or below the ocular limit λ/p , that is, the smallest angular distance that can be resolved by an aperture p . The answer is in the affirmative, because we have already supposed that $w_2 s$ exceeds λ . The slit has thus a visible width, and it is seen backed by undiffracted sunshine. If a spectrum be now formed by the use of dispersion sufficient to give a prescribed degree of purity, it is as bright as is possible with the sun as ultimate source, and would be no brighter even were the solar diameter increased, as it could in effect be by the use of a burning-glass throwing a solar image upon the slit. The employment of a telescope in the formation of the spectrum gives no means of escape from this conclusion. The precise definition of the brightness of any part of the resulting spectrum would give opportunity for a good deal of discussion; but for the present purpose it may suffice to suppose that, if the spectrum is to be divided into n distinguishable parts, so that its angular width is n times the angular width of the slit, the apparent brightness is of order $1/n$ as compared with that of the sun.

Under the conditions above supposed the angular width of the slit is in excess of the ocular limit, and the distance might be increased beyond d without prejudice to the brilliancy of the spectrum. As the angular width decreases, so does the angular dispersion necessary to attain a given degree of purity. But this process must not be continued to the point where w_2/d approaches the ocular limit. Beyond that limit it is evident that no accession of purity would attend an increase in d under given dispersion. Accordingly the dispersion could not be reduced, if the purity is to be maintained; and the brightness necessarily suffers. It must always be a condition of full brightness that the angular width of the slit exceed the ocular limit.

Let us now suppose, on the other hand, that w_2 is so small

* About 30 inches.

that the image of the sun is dilated to many times s , or that w_2 is much less than λ/s . The divergence of the light is now not s , but λ/w_2 ; and, if the pupil be just immersed,

$$p'd = \lambda/w_2.$$

The angular width of the slit w_2/d is thus equal to λ/p , that is, it coincides with the ocular limit. The resulting spectrum necessarily falls short of full brightness, for it is evident that further brightness would attend an augmentation of the solar diameter, up to the point at which the dilatation due to diffraction is no longer a sensible fraction of the whole. In comparison with full brightness the actual brightness is of order $w_2 s \lambda$; or, if the purity required is represented by n , we may consider the brightness of the spectrum relatively to that of the sun to be of order $w_2 s/(n\lambda)$.

In the application of these considerations to Lloyd's bands we have to regard the narrow slit w_2 as illuminated, not by the sun of diameter s , but by the much narrower source allowed by the first slit, whose angular width is w_1/D . On this account the reduction of brightness is at least $w_1/(sD)$. If w_1 be so narrow as itself to dilate the solar image, a further reduction would ensue; but this could always be avoided, either by increase of D , or by the use of a burning-glass focusing the sun upon the first slit. The brightness of the spectrum of purity n from the second slit is thus of order

$$\frac{w_1}{sD} \cdot \frac{w_2 s}{n\lambda} = \frac{w_1 w_2}{n\lambda D}.$$

We have now to introduce the limitations upon w_1 and w_2 . By (4) w_1 must not exceed $b/(4n)$; and by (2) w_2 must not exceed $\lambda D/(2b)$. Hence the brightness is of order

$$1/(8n^2), \quad . \quad . \quad . \quad . \quad . \quad . \quad (7)$$

independent of s , and of the linear quantities. The fact that the brightness is inversely as the *square* of the number of bands to be rendered visible explains the somewhat sudden failure observed in experiment. If $n=2000$, the original brightness of the sun is reduced in the spectrum some 30 million times, beyond which point the illumination could hardly be expected to remain sufficient for vision of difficult objects such as narrow bands.

In Fresnel's arrangement, where the light is reflected perpendicularly from two slightly inclined mirrors, interference of high order is obtained by the movement of one of the mirrors parallel to its plane. The increase of n does not then entail a narrowing of w_1 ; and bands of order n may be

observed in the spectrum of light transmitted through w_2 , whose brightness is proportional to n^{-1} , instead of, as before, to n^{-2} .

Achromatic Interference-Bands.

We have already seen from (3) that in the ordinary arrangement, where the source is of white light entering through a narrow slit, the heterogeneity of the light forbids the visibility of more than a few bands. The scale of the various band-systems is proportional to λ . But this condition of things, as we recognize from (2), depends upon the constancy of b , that is, upon the supposition that the various kinds of light all come from the same place. Now there is no reason why such a limitation should be imposed. If we regard b as variable, we recognize that we have only to take b proportional to λ , in order to render the band-interval (Λ) independent of the colour. In such a case the *system* of bands is achromatic, and the heterogeneity of the light is no obstacle to the formation of visible bands of high order.

These requirements are very easily met by the use of Lloyd's mirror, and of a diffraction-grating, with which to form a spectrum. White light enters the dark room through a slit in the window-shutter, and falls in succession upon a grating, and upon an achromatic lens, so as to form a real diffraction-spectrum, or rather series of such, in the focal plane. The central image, and all the lateral coloured images except one, are intercepted by a screen. The spectrum which is allowed to pass is the proximate source of light in the interference experiment; and since the deviation of any colour from the central white image is proportional to λ , it is only necessary so to arrange the mirror that its plane passes through the white image in order to realize the conditions for the formation of achromatic bands.

There is no difficulty in carrying out the experiment practically. I have used the spectrum of the second order, as given by a photographed grating of 6000 lines in an inch, and a photographic portrait lens of about 6 inches focus. At a distance of about 7 feet from the spectrum the light fell upon a vertical slab of thick plate-glass 3 feet in length and a few inches high. The observer upon the further side of the slab examines the bands through a Coddington lens of somewhat high power, as they are formed upon the plane passing through the end of the slab. It is interesting to watch the appearance of the bands as dependent upon the degree in which the condition of achromatism is fulfilled. A comparatively rough adjustment of the slab in azimuth is sufficient

to render achromatic, and therefore distinct, the first 20 or 30 bands. As the adjustment improves, a continually larger number become visible, until at last the whole of the doubly illuminated field is covered with fine lines.

In these experiments the light is white, or at least becomes coloured only towards the outer edge of the field. By means of a fine slit in the plane of the spectrum we may isolate any kind of light, and verify that the band-systems corresponding to various wave-lengths are truly superposed.

When the whole spectrum was allowed to pass, the white and black bands presented so much the appearance of a grating under the microscope that I was led to attempt to photograph them, with the view of thus forming a diffraction-grating. Gelatine plates are too coarse in their texture to be very suitable for this purpose; but I obtained impressions capable of giving spectra. Comparison with spectra from standard gratings showed that the lines were at the rate of 1200 to the inch. A width of about half an inch (corresponding to 600 lines) was covered, but the definition deteriorated in the outer half. A similar deterioration was evident on direct inspection of the bands, and was due to some imperfection in the conditions—perhaps to imperfect straightness of the slab. On one occasion the bands were seen to lose their sharpness towards the middle of the field, and to recover in the outer portion.

With respect to this construction of a grating by photography of interference-bands, a question may be raised as to whether we are not virtually copying the lines of the original grating used to form the spectrum. More may be said in favour of such a suggestion than may at first appear. For it would seem that the case would not be essentially altered if we replaced the *real* spectrum by a *virtual* one, abolishing the focusing lens, and bringing Lloyd's mirror into the neighbourhood of the grating. But then the mirror would be unnecessary, since the symmetrical spectrum upon the other side would answer the purpose as well as a reflexion of the first spectrum. Indeed, there is no escape from the conclusion that a grating capable of giving on the two sides similar spectra of any one order, without spectra of other orders or central image, must produce behind it, without other appliances and at all distances, a system of achromatic interference-fringes, which could not fail to impress themselves upon a sensitive photographic plate. But a grating so obtained would naturally be regarded as merely a copy of the first.

Another apparent anomaly may be noticed. It is found in

practice that, to reproduce a grating by photography, it is necessary that the sensitive plate be brought into close contact with the original; whereas, according to the argument just advanced, no such limitation would be required.

These discrepancies will be explained if, starting from the general theory, we take into account the actual constitution of the gratings with which we can experiment. If plane waves of homogeneous light (λ) impinge perpendicularly upon a plane ($z=0$) grating, whose constitution is periodic with respect to x in the interval σ , the waves behind have the general expression

$$\begin{aligned} A_0 \cos (kat - kz) + A_1 \cos (px + f_1) \cos (kat - \mu_1 z) \\ + B_1 \cos (px + g_1) \sin (kat - \mu_1 z) \\ + A_2 \cos (2px + f_2) \cos (kat - \mu_2 z) + \dots; \quad (8) \end{aligned}$$

where

$$p = 2\pi/\sigma, \quad k = 2\pi/\lambda,$$

and

$$\mu_1^2 = k^2 - p^2, \quad \mu_2^2 = k^2 - 4p^2, \quad \&c.,$$

the series being continued as long as μ is real*. Features in the wave-form for which μ is imaginary are rapidly eliminated. For the present purpose we may limit our attention to the first three terms of the series, which represent the central image and the two lateral spectra of the first order.

When the first term occurs, as usually happens, the phenomena are complicated by the interaction of this term with the following ones, and the effect varies with z in a manner dependent upon λ . This is the ordinary case of photographic reproduction, considered in the paper referred to. If A_0 vanish, there is no central image; but various cases may still be distinguished according to the mutual relations of the other constants. If only A_1 , or only B_1 , occur, we have interference-fringes. The intensity of light is (in the first case)

$$A_1^2 \cos^2 (px + f), \quad . \quad . \quad . \quad . \quad . \quad (9)$$

vanishing when

$$px + f = \frac{1}{2}(n+1)\pi;$$

and these fringes may be regarded as arising from the interference of the two lateral spectra of the first order,

$$\begin{aligned} \frac{1}{2}A_1 \cos (kat - \mu_1 z + px + f_1), \\ \frac{1}{2}A_1 \cos (kat - \mu_1 z - px - f_1). \end{aligned}$$

As an example of only one spectrum, we may suppose

$$B_1 = A_1, \quad g_1 = f_1 - \frac{1}{2}\pi,$$

* Phil. Mag. March 1881; *Enc. Brit.* Wave Theory, p. 440.

giving

$$A_1 \cos (kat - \mu_1 z - px - f_1). \quad . \quad . \quad . \quad (10)$$

A photographic plate exposed to this would yield no impression, since the intensity is constant.

In order, then, that a grating may be capable of giving rise to the ideal system of interference-fringes, and thus impress itself upon a sensitive plate at any distance behind, the vibration due to it must be of the form

$$A \cos (\mu x + f) \cos (kat - \mu_1 z). \quad . \quad . \quad . \quad (11)$$

It does not appear how any actual grating could effect this. Supposing $z=0$, we see that the amplitude of the vibration immediately behind the grating must be a harmonic function of x , while the phase is independent of x , except as regards the reversals implied in the variable sign of the amplitude. Gratings may act partly by opacity and partly by retardation, but the two effects would usually be connected; whereas the requirement here is that at two points the transmission shall be the same while the phase is reversed.

We can thus hardly regard the interference-bands obtained from a grating and Lloyd's mirror as a mere reproduction of the original ruling. As will be seen in the following paragraphs, much the same result may be got from a prism, in place of a grating; and if the light be sufficiently homogeneous to begin with, both these appliances may be dispensed with altogether.

Prism instead of Grating.

If we are content with a less perfect fulfilment of the achromatic condition, the diffraction-spectrum may be replaced by a prismatic one, so arranged that $d(\lambda/b)=0$ for the most luminous rays. The bands are then achromatic in the same sense that the ordinary telescope is so. In this case there is no objection to a merely virtual spectrum, and the experiment may be very simply executed with Lloyd's mirror and a prism of (say) 20° held just in front of it.

The number of black and white bands to be observed is not so great as might perhaps have been expected. The lack of contrast which soon supervenes can only be due to imperfect superposition of the various component systems. That the fact is so is at once proved by observation according to the method of Fizeau; for the spectrum from a slit at a very moderate distance out is seen to be traversed by bands. If the adjustment has been properly made, a certain region in the yellow-green is uninterrupted, while the closeness of the bands increases towards either end of the spectrum. So far

as regards the red and blue rays, the original bands may be considered to be already obliterated, but so far as regards the central rays, to be still fairly defined. Under these circumstances it is remarkable that so little colour should be apparent on direct inspection of the bands. It would seem that the eye is but little sensitive to colours thus presented, perhaps on account of its own want of achromatism.

It is interesting to observe the effect of coloured glasses upon the distinctness of the bands. If the achromatism be in the green, a red or orange glass, so far from acting as an aid to distinctness, obliterates all the bands after the first few. On the other hand, a green glass, absorbing rays for which the bands are already confused, confers additional sharpness. With the aid of a red glass a large number of bands are seen distinctly, if the adjustment be made for this part of the spectrum.

A still better procedure is to isolate a limited part of the spectrum by interposed screens. For this purpose a real spectrum must be formed, as in the case of the grating above considered.

We will now inquire to what degree of approximation λ/b may be made independent of λ with the aid of a prism, taking Cauchy's law of dispersion as a basis. According to it the value of b for any ray may be regarded as made up of two parts—one constant, and one varying inversely as λ^2 . We therefore write

$$\frac{\lambda}{b} = \frac{\lambda^3}{A\lambda^2 - B}, \quad \dots \dots \dots (12)$$

where A is to be so chosen that λ/b is stationary when λ has a prescribed value, λ_0 . This condition gives

$$A\lambda_0^2 = 3B; \quad \dots \dots \dots (13)$$

so that

$$\frac{\lambda/b}{\lambda_0/b_0} = \frac{2}{3\lambda_0} \frac{\lambda^3}{\lambda^2 - \frac{1}{3}\lambda_0^2}. \quad \dots \dots \dots (14)$$

As an example, let us suppose that the disposition is achromatic for the immediate neighbourhood of the line D, so that $\lambda_0 = \lambda_D$, and inquire into the proportional variation of λ/b , when we consider the ray C. Assuming

$$\lambda_D = \cdot 58890, \quad \lambda_C = \cdot 65618,$$

we obtain from (14)

$$\frac{\lambda/b}{\lambda_0/b_0} = 1\cdot 0155.$$

The meaning of this result will be best understood if we inquire for what order (n) the bands of the C-system are

shifted relatively to those of the D-system through half the band-interval. From (1)

$$\begin{aligned}\delta u &= nD\{\lambda/b - \lambda_0/b_0\} \\ &= \frac{1}{2}\lambda_0 D/b_0\end{aligned}$$

by hypothesis ; so that

$$n = \frac{\frac{1}{2}\lambda_0/b_0}{\lambda/b - \lambda_0/b_0} \dots \dots \dots (15)$$

Thus, in the case supposed, $n=32$. After 32 periods the black places of the C-system will coincide with the bright places of the D-system, and conversely. If no prism had been employed (b constant), a similar condition of things would have arisen when

$$n = \frac{\frac{1}{2}\lambda_0}{\lambda - \lambda_0} = 4.2.$$

If $(\lambda - \lambda_0)$ or, as we may call it, $\delta\lambda$ be small,

$$\frac{\lambda/b - \lambda_0/b_0}{\lambda_0/b_0}$$

is of the second order in $\delta\lambda$. An analytical expression is readily obtained from (14). We have

$$\begin{aligned}\frac{\lambda/b}{\lambda_0/b_0} &= \frac{1 + 3\delta\lambda/\lambda_0 + 3(\delta\lambda/\lambda_0)^2 + (\delta\lambda/\lambda_0)^3}{1 + 3\delta\lambda/\lambda_0 + \frac{3}{2}(\delta\lambda/\lambda_0)^2} \\ &= 1 + \frac{\frac{3}{2}(\delta\lambda/\lambda_0)^2 + (\delta\lambda/\lambda_0)^3}{1 + 3\delta\lambda/\lambda_0 + \frac{3}{2}(\delta\lambda/\lambda_0)^2} \\ &= 1 + \frac{3}{2}(\delta\lambda/\lambda_0)^2 - \frac{7}{2}(\delta\lambda/\lambda_0)^3,\end{aligned}$$

approximately ; so that, by (15),

$$n = \frac{1}{3} \left(\frac{\lambda_0}{\delta\lambda} \right)^2 \left\{ 1 + \frac{7}{3} \frac{\delta\lambda}{\lambda_0} + \dots \right\} \dots \dots (16)$$

This gives the order of the band at which complete discrepancy first occurs for λ_0 and $\lambda_0 + \delta\lambda$, the adjustment being made for λ_0 . It is, of course, inversely proportional to the *square* of $\delta\lambda$, when $\delta\lambda$ is small.

The corresponding value of n , if no prism be used, so that b is constant, is

$$n = \frac{1}{2} \frac{\delta\lambda}{\lambda_0} \dots \dots \dots (17)$$

The effect of the prism is thus to increase the number of bands in the ratio

$$2\lambda_0 : 3\delta\lambda.$$

[To be continued.]

XII. *Note on some Photographs of Lightning and of "Black" Electric Sparks.* By A. W. CLAYDEN*.

DURING the thunderstorm on the night of June 6 I exposed several plates in the hope of securing photographs of lightning. Three of these gave results.

One was exposed to two flashes, not counting such as did not cross the field of view. These two flashes show complicated and beautiful structure. One of them is a multiple flash, distinctly seen as double by both my wife and myself. An enlargement of this shows curious flame-like appendages pointing upwards from every angle. The other flash is a broad ribbon. The images of the masonry in the left-hand corner (which are necessarily slightly out of focus) show three positions of the camera. They are sharp, hence the camera did not move during the existence of a flash; and the directions of those movements which did occur do not in any way correspond to the movements (if such there were) which would have been required to produce the ribbon-like effect from a linear flash.

A second plate shows four flashes, and the camera moved much more than in the first case. None of these flashes are ribbons. Development showed the plate to be overexposed.

The third was exposed to six flashes; that is to say, I judged that six of them crossed the field of view. There were many others between times, which were either in the clouds or occurred in other parts of the sky. One flash, I remarked at the time, must be "right down the middle of the plate." Development showed this plate to be very much overexposed, and the image required careful nursing. I was much surprised to see nothing but one triple flash in the corner. I supposed that I must have mistaken the plate, and was about to throw it away, but on carefully searching for the above-mentioned vertical flash, I found its image was reversed, printing as a black flash with a white core. Subsequent observation showed other dark flashes; and the enlargement of part of the plate shows that there are indications of white cores to each of them.

Now the connexion between this reversal and overexposure was very striking. Hence it occurred to me that the black flashes might be due to a sort of cumulative action. Not to the excessive brightness of the individual flashes, but rather to the excessive action produced by the superposition of the

* Communicated by the Physical Society: read June 22, 1889.

glare from an illuminated white cloud upon the normal image of the flash.

To test this I endeavoured to obtain the same effect with the sparks from a small Wimshurst machine ; but, under the conditions in which I worked, I could not get a longer spark than one inch.

I first photographed a series of brilliant sparks, using two large Leyden jars. These gave normal images, very dense, and shaded off at the margins, although the focus, as shown by the knobs of the machine, was good.

Next I tried less brilliant sparks from the machine with its ordinary small jars. These gave similar images, but less dense.

Then I repeated both experiments, and before developing the plates exposed them to the diffused light from a gas-flame. The brilliant sparks then yielded images which may either be called normal with a reversed margin, or reversed with a normal core. The fainter sparks were completely reversed.

One plate of bright sparks was exposed to the gas-light, so that different parts were acted upon for different times. The reversal seems to spread inwards as the exposure to diffused light is increased.

One plate of faint sparks was only half of it exposed to diffused light. The result is that on that part the sparks are reversed, while on the other they are normal.

Finally I photographed a number of sparks in a series across the plate, and placed a sheet of white cardboard behind them to do duty for the white background of cloud. Some of the first sparks impressed on the plate show reversed images.

Coupling these experiments with the observations as to the overexposure of the "dark-flash" plate, and with the fact that all dark-flash plates I have seen show symptoms of considerable exposure, I submit that there is at least a good case for this theory of cumulative or repeated action producing the reversal. The partial reversal of the bright sparks seems to correspond with the bright core to some dark flashes; and the complete reversal of the less brilliant sparks to the absence of any such core from the less conspicuous portions of a dark flash.

There is certainly one difficulty yet to be got over, and that is the crossing of a dark flash by a bright one. However, I have some experiments* in view which I hope may throw some

* Since writing the above communication I have made a number of further experiments, which I hope to describe in detail at some future time. But perhaps I may be allowed to say at once that I have succeeded in imitating the phenomenon of a bright image crossing a dark

light upon this also. In my own negative the point of crossing seems to be extra bright.

Meanwhile I must apologize to the Society for bringing forward these notes in such an immature and hastily constructed condition. My excuse must be that the photographs of electric sparks were only taken the day before yesterday, and today's meeting is the last of the session.

XIII. *Expansion with Rise of Temperature of Wires under Pulling Stress.* By J. T. BOTTOMLEY, M.A., F.R.S., F.C.S.*

[Plate IX.]

IT is probably well known to the members of the Physical Society that, at the instance of the British Association and with the assistance of a money grant from that body, very interesting secular experiments on the elasticity and ductility of wires were commenced some years ago in Glasgow. In the tower of the Glasgow University buildings certain wires are hung in pairs for comparison. One of each pair carries a heavy load about half the breaking weight of the wire; the other carries about one tenth of the breaking weight. Certain marks are put on the wires; and the object of the experiment is to find whether the heavily loaded wire seems, on comparison with the lightly loaded wire, to go on running down incessantly, or whether it comes asymptotically to a fixed length for a given temperature, ceasing to experience further permanent elongation.

The observations of the last few years show that the elongation due to further pulling out has, to say the least, become exceedingly small, so small that it is extremely difficult to observe it; and at the Aberdeen meeting of the British Association I pointed out that a great difficulty is introduced into the making of deductions from these observa-

one. The experiments point to the conclusion that diffused light acting upon a plate can reverse previously impressed images of electric sparks, but is powerless to affect any such impressions which may be made afterwards. Similar results are obtained whether the source of the diffused light is a gas-flame, a lamp, or a series of sparks. I do not at present offer any theoretical explanation of these facts, but they are in themselves sufficient from a meteorological point of view. "Dark" flashes of lightning have no existence in nature, but are caused by the exposure of the plate to an illuminated sky *after* the passage of the flash. This illumination may be due to subsequent flashes, the more recent of which will give normal images possibly crossing the reversed ones.

* Communicated by the Physical Society: read June 22, 1889.

tions through the impossibility of controlling the temperature of the tube in which the wires are placed. If, for example, there is any difference as to expansion with temperature of the same wire when lightly and when heavily loaded, a cause of disturbance would be introduced which it would be excessively difficult to allow for. It seemed therefore absolutely essential to make direct experiments on this point. The object of the present communication is to give an account of some experiments of this kind. A preliminary account of these experiments was communicated to the British Association at the Manchester meeting (1887), and was printed in the *Philosophical Magazine* for October of that year.

The wires hung up in the tower of the Glasgow University building are two of platinum, two of gold, and two of palladium, these wires being chosen on account of their small liability to oxidation. The wires on which I have experimented up to the present time have, however, been of copper and platinoid. The latter metal is an alloy* of nearly the same composition as German silver, but containing a small quantity of tungsten and made in a peculiar way.

The figures show the arrangements for experimenting. A long tube of tin-plate about $2\frac{1}{2}$ inches in diameter was set up vertically, fixed by means of brackets at two or three places. This tube has inlets and outlets for steam, of which I have a plentiful supply in the laboratory from boilers connected with the University apparatus for heating and ventilation. It has also openings for thermometers. The length of the tube was $17\frac{1}{2}$ feet in the experiment with copper wire, and somewhat shorter in the platinoid experiment.

A piece of excellent copper wire was taken, and its breaking weight was found to be 750 grammes. Its diameter was 0.22 millim. Two portions of this wire were hung side by side in the centre of the tube. In order to suspend them their ends were passed into two small trumpeted holes in a stout brass plate and soldered to the back of the plate. The plate was screwed up to a strong beam in the ceiling of the laboratory. This forms by far the best mode of supporting a wire for experiments on elasticity. One of the wires carried 75 grammes, the other 375 grammes.

A few preliminary experiments as to heating and cooling revealed a difficulty the magnitude of which I was unprepared for. When the steam was admitted into the tube the wires of course expanded, the heavily loaded wire going down far more than the other; and when the steam had been stopped and the tube allowed to cool, they contracted again

* Invented and patented by Mr. F. W. Martino of Sheffield.

but not to the same extent; and neither came back to its original length. This was of course to be expected. But it turned out, on repeating the heating and cooling, that the same thing occurred again and again; and it was not till after about 150 heatings and coolings that the heavily loaded wire assumed a permanent state, expanding and contracting by equal amounts with the heating and cooling*. The lightly loaded wire took its permanent condition much sooner. This itself was a valuable result, applying directly to the early observations on the secular wires in the University tower.

Fig. 1, Pl. IX., shows the arrangement for these preliminary experiments. Behind the wire a half-millimetre scale was put up; and each wire carried a pointer moving over the scale. The readings at hot and cold temperatures were taken with the well-known Quincke microscope-kathetometer; and the process and observations were carried on, as has been said, till each pointer gave unvarying readings at the hot and cold temperatures. It was then considered that the wires had assumed a permanent condition.

The pointers and scale were now removed and two hooks, of peculiar construction, figs. 2 and 3, were attached to the ends of the wire, the wires being passed into holes made for the purpose and soldered in. These hooks carried and formed part of the stretching weights. The upper parts of the hooks are turned over to form two horizontal plates, and the vertical parts of the hooks press very lightly against each other and form almost frictionless guides one for the other. In one of the vertical faces a vertical V-groove is cut, while the remainder of the face is plane and well-polished. Two little feet on the vertical face of the other hook move in the V-groove of the first, and a third foot rests against the smooth vertical face. A relative geometrical guide is thus provided for the hooks, and the shape of the hooks is such that the gravity of the whole, including the weights, gives the requisite slight pressure of the one against the other. The horizontal parts of the hooks just mentioned carry what is practically a small three-legged table, of which two legs rest on one platform and the third on the other. To be more precise, one of the platforms carries on its top a little plate with a V-groove cut in it; and a knife-edge, cut away at the central parts and thus leaving two feet at the extremities, attached

* I must not fail to express here my indebtedness to Mr. Thomas A. B. Carver, assistant, and Mr. W. S. Cook, student in the Physical Laboratory, who carried out these experiments in the winter sessions 1887-8 and 1888-9 respectively. Without their patient labour the work would have been impossible to me.

to the table, rests in the V. A third foot, rounded, rests on the other platform, which is plane and polished. On the top of this little table, which is a square of about $1\frac{1}{2}$ centimetre in the side, there is fastened a perfectly plane parallel Steinhilber mirror; and a telescope with cross wires, looking down very nearly vertically on the mirror, views, reflected in the mirror, a half-millimetre scale suitably placed.

It will be seen at once that if the two wires were to elongate equally with rise of temperature, their extremities would go down together and almost the only effect (not absolutely of course) on the scale-reading would be to alter somewhat the focus. But if one wire elongates more than the other the mirror is tilted, and the change in the scale-reading readily gives the amount of relative displacement of the ends of the wire.

The arrangement works in the most satisfactory way, and it now only remains for me to state the results. I must remark, however, that it was exceedingly difficult to make an exact estimate of the temperature of the tube, even when the steam was running strongly through it. Thermometers inserted by means of corks in holes provided for the purpose showed that differences of 2° or 3° (I think not so much as 5° , however) existed at different parts of the tube.

This being understood, I may say that the range of temperature in the various experiments was from 15° C. or 16° C. (cold) to 98° or 99° (hot), or about 83° C. The length of copper wire experimented on was 530 centimetres. The difference of expansions observed was 0.14 millim. or 0.014 centimetre, the heavily loaded wire going down most. This gives a relative expansion of 26×10^{-6} per centimetre for a change of temperature of 83° ; or 0.314×10^{-6} per centimetre per degree.

I find the linear expansion of copper per degree stated at about 17.2×10^{-6} , and thus the ratio of this extra expansion to the total expansion is $3.14/172$, or about $\frac{1}{55}$.

With regard to platinoid wire—after more than three months of daily heating and cooling, the wires (0.35 millim. in diameter) came to a thoroughly permanent condition. On a length of 490 centimetres a relative extra extension of 0.111 millim. or 0.011 centim. was observed for a change of temperature of 83° , and as with copper the heavily loaded wire experienced most elongation. These numbers give 22.4×10^{-6} as the extra expansion per centimetre, or 0.27×10^{-6} per centimetre per degree Centigrade.

The linear expansion of platinoid was unknown, though it might be supposed to be something not very different from that

of German silver. Accordingly a series of experiments were carried out on this question, with the result that the linear expansion of the specimen used was found to be $\cdot 0000154$ per degree Centigrade.

The relative extra expansion of platinoid wire is therefore $\frac{2\cdot 7}{154}$, or $\frac{1}{57}$.

XIV. *A Determination of the Value of the B.A. Unit of Resistance in Absolute Measure, by the Method of Lorenz.*
By Dr. LOUIS DUNCAN, GILBERT WILKES, and CARY T. HUTCHINSON*.

THIS work was done at the Physical Laboratory of Johns Hopkins University during the spring of 1888. Lord Rayleigh's modification of Lorenz's original method was used. In this, as is well known, a measured part of the current flowing through the inducing coils is balanced by the current induced by the rotation of the disk.

The apparatus employed is that designed by Prof. Rowland for his determination of the ohm undertaken for the United States Government. A detailed description of it is contained in his forthcoming report, so only a few words will be given to it here. The induction-coils, four in number, were wound in square channels cut in heavy flanges, which were cast on the exterior of a hollow brass cylinder open at both ends. The coils were respectively 30·171, 9·786, 10·545, and 30·775 centim. from the mean plane of the disk, itself placed as nearly as possible midway between the ends. The cylinder is about 66 centim. long, 100 centim. in diameter, and 1 centim. thick. It is thus the longest ever used in work of this kind. The flanges and cylinder were cast in one piece, and the tooling was all done without removing the casting from the lathe. The walls of the channels were left very thick to prevent spreading during the winding of the coils. The radius of the disk was so chosen that an error in its value should enter as slightly as possible in the value of the coefficient of induction.

The disk was brass, 21·5 centim. radius and $\cdot 5$ centim. thick. It was fixed to a brass axle, 3 centim. diameter, turning in bearing-boxes carried by suitable framework fixed inside the cylinder. There was a cone of grooved pulleys toward one end of the axle, used for getting different speeds of the disk. The motor for running the disk was in the

* Communicated by the Authors.

adjoining room, about 10 metres from the disk. The speed obtained varied from 26 to 47 revolutions per second, higher than has usually been used.

The current was taken from the edge of the disk by three brushes which bore on it at angular distances of 120° ; each brush was made of three or four brass strips of different lengths soldered together at one end; each strip in every brush touched the disk, one brush occupying a length of 2 centim. or more on the edge. The strips were made of various lengths in order to avoid systematic vibrations. For the contact at the centre, a conical counterboring was made in one end of the axle and a brass point was pressed into it constantly by a stiff spring. The counterboring in the axle, the point, the brushes, and the edge of the disk were all carefully amalgamated before each observation; particular care was given to this. The insulation resistance of the coils was found to be from six to ten megohms.

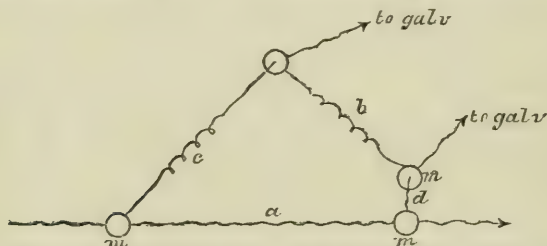
The arrangement for getting the speed differed from that generally employed. As the quantity desired is the average speed during the time of an observation, it seemed that a chronograph, if sufficiently accurate, would give this better than any other means, besides furnishing at a glance the history of the systematic variations of the speed, while the galvanometer showed the abrupt changes. The spot of light of the galvanometer was usually very steady, showing that there were no sudden changes. Every hundredth revolution of the disk was recorded on the chronograph. To accomplish this, one end of the axle was connected to an ordinary speed-counter, consisting of a worm wheel and endless screw, which rested on a board fixed to receive it. The worm wheel carried a small brass pin, which made contact every revolution with a brass strip fixed near it, thus closing the chronograph circuit. The strip was adjustable and the contact was always made as slight as possible consistent with certainty. The duration of this contact was about $\frac{1}{80}$ sec., while the clock-break was nearly twice this.

The connexion of the axle with the endless screw was made in this way:—A small hard rubber screw with square head was fitted in the end of the axle and was joined to the screw of the speed-counter by drawing over both a piece of pure rubber tubing with thick walls, about 2 centim. long. This connexion is easily made, permits no slipping, and absorbs vibrations so completely that even for comparatively high speeds no fastening is required to hold the counter down to the board; but for the very high speeds we used it was necessary to secure it to the rubber bed on which it lay by

rubber bands, in order to ensure perfectly uniform contact between the pin and the spring.

The chronograph was a large and excellent instrument by Fauth; the drum was about 18 centim. diam., and in this work revolved in 30 seconds; the length of a second was thus nearly 2 centim.; the sheet could be read with rough means to $\frac{1}{2}$ millim. ($=\frac{1}{40}$ sec.); and was actually read much closer. As each observation lasted five minutes, even this gave an estimation of the mean speed to $\frac{1}{6000}$.

The galvanometer was a low resistance one of the Thomson reflecting type; a small piece of wire which dipped in a light oil was hung from the needle and acted as a damper: with this the needle was found to be sufficiently sensitive, and to come nearly to rest in about twelve seconds after reversing the current through it. The resistance a in the figure through



which the main current flows is a large 1-ohm coil of German silver wound about a skeleton cylinder of glass rods, and is about 30 centim. high and 15 centim. in diameter. The ends of the coil are soldered to copper blocks which form the bottoms of mercury-cups. It is placed in an earthenware jar filled with special light oil known to be a good insulator, and is provided with a stirring-paddle. Resistance b is a 1-ohm coil, by Elliot, of the usual form; this is put in a large glass jar and surrounded with water. Resistance c is taken from specially made "comparators;" each consists of ten coils of the same nominal value wound together on a copper cylinder 6 centim. diameter; they are properly insulated &c., and protected by a larger concentric cylinder. The terminals are soldered to the copper bottoms of mercury-cups arranged in two circles around the hard rubber ring which closes the annular space between the inner and outer copper cylinders. The inner cylinder is filled with water. The connexions of the ten coils can be varied at pleasure; they can all be thrown in series, in parallel, or in any intermediate arrangement. There were two comparators used, with the coils 100 and 10 ohms respectively.

To keep the temperature constant, spirals of lead pipe were placed round the Elliot coil and in the inner cylinders of the comparators, through which there was a constant flow of water from the city supply. This answered its purpose admirably: the temperature varied only a degree or so even from day to day. The water was of course allowed to flow some hours before beginning observations.

The terminals of all resistances were brought to large mercury-cups, m, m , each having an amalgamated copper disk lying on the bottom. The main current did not flow through any part of the circuit of the induced current; a short bridging-piece, d , is used, as Lord Rayleigh found necessary.

The resistances used were all compared several times by different observers with the standard: this was a Warden Muirhead 10-ohm coil, whose value was determined at the Cavendish Laboratory in 1887; it was 9.99416 B.A. units at $16^{\circ}5$, with temperature-coefficient of .000292 per ohm per degree.

In taking the observations, the aim was to adjust the resistances first so that there should be only a small deflexion. After a number of galvanometer-readings for this "balanced" arrangement had been taken, the resistance c was changed so as to give a deflexion of ten divisions (say); readings were taken for this "unbalanced" arrangement; the original "balanced" was then restored and readings taken. If nothing had changed sensibly since the beginning of the experiment the average deflexions for the two "balanced" would agree: of course this condition was only approximated to. The "unbalanced" set gives the data for correcting for the small deflexion of the "balanced."

Each experiment then consists of the galvanometer-, speed-, temperature-, &c. readings pertaining to the three arrangements of resistances: these three arrangements are called A, B, and A', in the order taken, irrespective of the magnitude of the deflexions. In general R_1 and R_3 (subscripts 1, 2, 3 refer to A, B, A' respectively; R is the "effective" resistance) are the same, and the corresponding deflexions are small; B is in this case used to correct both R_1 and R_3 , and the mean of the corrected values is used. When, however, the deflexion for A happens to be undesirably large after beginning the experiment, B is made to give a small deflexion, and A' made as nearly as may be the same as A. We have in this case to apply the mean of two corrections to R_2 , one from A and the other from A'.

In each arrangement, as A, the current is reversed four

times; it is kept in the same direction for one minute at a time, and five galvanometer-readings at equal intervals of time are taken each minute: this gives, then, twenty-five galvanometer-readings and occupies five minutes. The set B is begun as quickly as possible after A. The chronograph-record is started by dropping the pen on the revolving drum only a few seconds before the first galvanometer-reading, and an effort is made to use that portion of the record beginning exactly with the readings; the record is stopped at the instant of the last reading by lifting the pen. Temperature-readings are taken before, after, and often during the set. The resistance carrying the main current is constantly stirred, and the others frequently. After A' the temperature of the cylinder and disk is noted.

Variety was given to the different experiments by using different pairs of induction-coils, inner or outer, and by varying the speed and direction of rotation. Sometimes, too, an experiment was repeated with everything the same, except that the resistance c would be made up of different coils.

The coefficient of mutual induction for the two pairs of coils as used by Prof. Rowland are:—

$$\text{Coils 1+4,} \quad M = 60292.5.$$

$$,, \quad 2+3, \quad M = 102030.2.$$

$$\text{Diameter of disk} = 43.1334 \text{ at } 17^\circ.$$

Before beginning these experiments, the disk was slightly turned off in order to smooth the edge; the diameter was measured by two observers, and was found to be

$$43.1201 \text{ at } 17^\circ \text{ C.}$$

The formulæ expressing the effect on M of small changes in the quantities entering in its expression are,

$$\text{for 1+4,} \quad \frac{dM}{M} = .015 \frac{dA}{A} + 1.912 \frac{da}{a} - .927 \frac{db}{b};$$

$$\text{for 2+3,} \quad \frac{dM}{M} = -.95 \frac{dA}{A} + 2.12 \frac{da}{a} - .17 \frac{db}{b};$$

where A = mean radius of the coil,

a = radius of disk,

b = distances of mean planes of disk and coil.

The corrections, calculated by these formulæ, due to the change in a give

for 1 + 4, $M = 60257$ at 17°C. ,

for 2 + 3, $M = 101964$,,

Let ρ = ratio of the B.A. unit to the ohm,

$R = \frac{ab}{a+b+c}$ "effective" resistance,

N = number of revolutions per second,

$D = D_S - D_N$ = difference of mean deflexions,

for the two positions S and N of the reversing-key; *i. e.* D would be the mean deflexion for either direction of current, if no irreversible effects existed.

Then will

$$\frac{1}{\rho} = \frac{1}{MN_{1,3}} \left\{ R_{1,3} - \frac{\frac{N_{1,3}}{N_2} R_2 - R_{1,3}}{\frac{N_{1,3}}{N_2} D_2 - D_{1,3}} D_{1,3} \right\}, \quad \dots \quad (1)$$

and

$$\frac{1}{\rho} = \frac{1}{MN_2} \left\{ R_2 - \frac{\frac{N_{1,3}}{N_2} R_2 - R_{1,3}}{\frac{N_{1,3}}{N_2} D_2 - D_{1,3}} D_2 \right\}. \quad \dots \quad (2)$$

(1) is used when $D_1 < D_2$; (2) is used when $D_1 > D_2$.

The double subscripts, as $R_{1,3}$, means that the two quantities R_1 and R_3 are to be used in turn; that is to say, each formula above is really double: first we use the subscript 1's, and then the subscript 3's. It was found more convenient to calculate the values of ρ_1 and ρ_3 this way and average them than to apply an average correction. Indeed, when the speeds N_1 and N_3 are different, this is the only way.

The following table gives the data and results of these experiments; the (+) direction of rotation is zenith, north, nadir, south.

1. Descp.	2. Coils.	3. Rotation.	4. N.	5. D.	6. R.	7. Correction.	8. Corrected Resistance.	9. M.	10. ρ .
4 $\left\{ \begin{array}{l} A \\ B \\ A' \end{array} \right\}$	1 & 4	+	35.648 -701 -715	+ 474 - 671 + 604	-00220207 215435 220216	- -00002110 - 2225	-00218097 217991	60266	98620
7 $\left\{ \begin{array}{l} A \\ B \\ A' \end{array} \right\}$	2 & 3	+	-720 -966	-1076 + 260	365835 372638	- 0828	371810	101979	647
8 $\left\{ \begin{array}{l} A \\ B \\ A' \end{array} \right\}$	"	+	-752 -929 36.055	+1015 - 615 - 037	372859 369317 372859	- 3345	369514	"	658
9 $\left\{ \begin{array}{l} A \\ B \\ A' \end{array} \right\}$	"	-	-268 -162 -347	+ 524 +1251 + 799	372837 369392 372837	+ 1695	374532	"	725
10 $\left\{ \begin{array}{l} A \\ B \\ A' \end{array} \right\}$	"	+	-514 -462 -503	+ 216 +1439 + 194	376476 372933 376476	+ 2712 + 0513	375549 377007	"	758
11 $\left\{ \begin{array}{l} A \\ B \\ A' \end{array} \right\}$	"	+	35.373 -365 -497	+ 412 - 970 +1496	365589 367517 365589	+ 0559	366148	101974	490
12 $\left\{ \begin{array}{l} A \\ B \\ A' \end{array} \right\}$	"	+	-066 -134 34.959	- 819 + 164 -1191	365680 363026 365680	+ 0560	363586	"	541
13 $\left\{ \begin{array}{l} A \\ B \\ A' \end{array} \right\}$	"	-	35.033 -999 -200	+ 053 + 710 - 249	362375 365710 362375	+ 0230	362145	"	724
14 $\left\{ \begin{array}{l} A \\ B \\ A' \end{array} \right\}$	"	-	34.249 -184	- 908 - 256	352865 352873	+ 1137 + 0267	363512	"	710
15 $\left\{ \begin{array}{l} A \\ B \\ A' \end{array} \right\}$	"	-	-621 -752 -844	-1488 + 081 - 884	352932 359721 357139	- 0282	359439	"	594
17 $\left\{ \begin{array}{l} A \\ B \\ A' \end{array} \right\}$	1 & 4	+	-999 -928 -715	+ 052 - 620 - 547	213697 215248 213697	+ 0154	213851	60232	710
						- 1939	211758		

Experiments Nos. 1, 2, 3, 6, 14A', 17A, and 22 were interrupted by divers accidents and never completed; in No. 19 there is confusion in the notes, making the sign of the deflexion doubtful; Nos. 21, 23, and 24 give values of ρ from 2 per cent. to 10 per cent. out, due to some error in the record of resistances used. This accounts for all the experiments begun.

The average of all the above is $\cdot 98622$; without No. 27 which differs about twice as much from the mean as any other observation, the average is $\cdot 98634$. The great divergence of No. 27 is in itself reason enough for giving it less weight; but in addition, the chronograph sheet shows that the speed here was very irregular, increasing, decreasing, and increasing again; this is the only occurrence of such irregularity. Therefore, giving it about one third weight, we find as the most probable value

$$1 \text{ B.A. Unit} = \cdot 9863 \text{ ohm.}$$

A determination of the "Mercury Unit" was recently made by Messrs. Hutchinson and Wilkes (Johns Hopkins University Circulars, May 1889; Phil. Mag. July 1889) who found the value to be $\cdot 95341$. Taking this with the above number for the B.A. unit, we have as the length of the mercury column corresponding to the ohm,

$$106\cdot 34 \text{ centims.}$$

XV. *Intermittent Lightning-Flashes.* By H. H. HOFFERT, D.Sc., A.R.S.M., *Demonstrator of Physics at the Normal School of Science and Royal School of Mines*.*

[Plate IV.]

THE storm which passed over London on the evening of June 6th afforded an unusually favourable opportunity for observations, both with and without the aid of the camera, on the character of lightning-flashes, and for determining the causes of some of the curious effects noticed by previous observers.

While watching the storm from my house in Ealing I could in several instances distinctly perceive a flickering appearance in a discharge, and in one particular case the repetitions were at least 5 or 6 in number, just sufficiently slow for the eye to detect the variations in brightness without removing the impression of one single flash. Other observers

* Communicated by the Physical Society: read June 8, 1889.

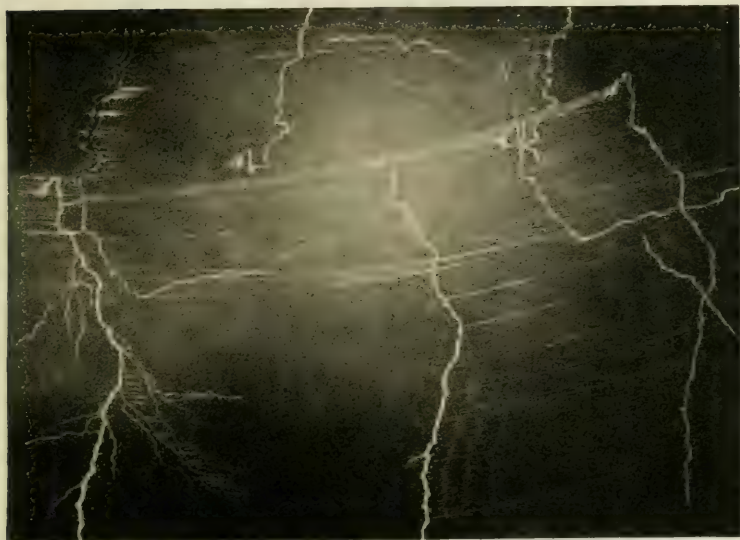


Fig. 1.

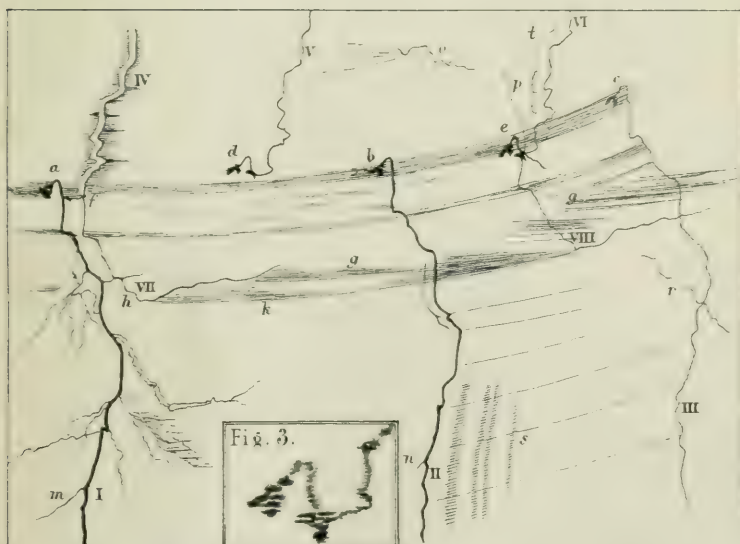


Fig. 2.

with whom I have since spoken have informed me that they had observed a similar effect, and that in some instances flashes, following as nearly as could be seen in the same path, were separated by an appreciable interval, often of several seconds' duration. Photographs of lightning have frequently been obtained showing banded, ribbon, or double flashes; but, so far as I am aware, these have not been of so decided a nature as to remove doubts whether the appearances could not be ascribed to the effects of halation by reflexion from the back of the negative, or to blurring from the photograph having been taken through the glass of a window, or to some other similar cause. I was therefore anxious to obtain some decisive evidence of the repetition of the flashes, and as my friend Mr. G. J. Snelus was at my suggestion attempting to obtain some photographs of the lightning, I joined him, and he kindly placed his camera and some plates at my disposal.

The rain having ceased for the moment, I was able to go out on to a balcony and thus get a good view of the storm, which had now approached quite close and seemed to surround us. The camera was held in the hand and pointed towards the north-east, where, about half a mile away, numerous brilliant flashes were occurring. The cap of the camera was taken off, and then the camera was moved in a horizontal plane about the lens as a centre at the rate of about once to and fro in three quarters of a second, until a flash was observed in the direction in which it was pointed, when the cap was at once replaced. The duration of the exposure of the plate was about half a minute. The camera was of quarter-plate size, the plates were Ilford rapid, and the lens, which was a rapid rectilinear, was used with full aperture ($f/8$).

I hoped, by having the camera moving, to be able to separate the successive components of the flashes, and in this I was fortunately successful. In fig. 1 (Pl. IV.) is a reproduction of one of the photographs obtained, and on it can be seen two triple flashes (I., II., III., and IV., V., VI.), and one double flash (VII. and VIII.).

During the interval that the plate was exposed the illumination of the sky from flashes out of the line of view, or behind clouds, produced the glare seen in the centre and upper part of the photograph, and some faint flashes which were noticed were probably the cause of the streaks seen in fig. 2 at *o*, *p*, and *r*.

A careful examination of the photograph reveals many interesting features. The three successive flashes I., II., III. are identical in form. If the negative be placed over a print so that either of the three lines on the negative lies over

either of those on the print, the coincidence is seen to be exact even to the smallest irregularities. Nevertheless, of all the branch-flashes which spread out from I. only a small trace exists at *n* in II., and none whatever in III. Sweeping across the photograph and connecting corresponding points in the successive flashes are streaks of light, showing that a very considerable residual illumination remains between the discharges. These streaks are especially well marked between the components of the double flash VII., VIII., and are also very bright along the path of the head of flash I. They are not always present; for in another photograph obtained upon a moving plate by Mr. Snelus, in which a flash is reduplicated, there is no trace of them, the flashes being quite sharp and distinct. The streaks commence abruptly with one discharge and end abruptly with another. Their extension to the left of I. was probably due to the camera not having quite reached the end of its swing. Where they are brightest there is in all cases a swelling at the part of the flash where they start.

At the upper left-hand corner there is a curious dark flash with bright edges and short luminous streaks. The form of this dark flash is exactly reproduced in the two bright flashes V. and VI. Dark flashes are frequently met with in photographs of lightning, and have been usually ascribed to reversal of the image by overexposure; but I do not think this explanation applies in the present case, both on account of the appearance which the dark flash presents, and also because as far as I can recollect the brilliant downward flash I. appeared to the eye much the brightest of those that occurred while the plate was exposed. There is a similar but less distinct dark gap to the left of VI.; and to the right of II. are three or four faint bands parallel to it and following its sinuosities. These all seem to be due to variations in brightness in the luminous streaks, which are thus shown to be electrical and not phosphorescent in character.

When carefully compared, the forms and positions of the flashes I. to VIII. are found to present such agreement as to show that they must have formed a system of discharges closely connected together. If V. be placed over IV. so as to coincide with its left-hand border, the portion *d* exactly coincides in form and position with the portion *a* of I., and both *a* and *d* terminate in a curious bifurcated enlargement which, when examined with a lens, shows a beaded appearance, roughly sketched in fig. 3. This coincidence of form and position would be difficult to account for if I. and IV. were independent flashes not occurring at the same time. It is

evident, however, that the series I., II., III. cannot have occurred during the same sweep of the plate as IV., V., VI., since the curve joining *a, b, c* is concave upwards, while that joining *a, d, e* is concave downwards. The flashes VII., VIII. seem also to have formed part of the same system of discharges, for the part of VII. from *f* to just above *h* is coincident with the upper portion of I., while the luminous streaks extending between VII. and VIII. agree in their directions with the curves joining *a, b, c* and *a, d, e*, those at *g* being parallel to the line joining *d* and *e*, and those at *k* following nearly but not exactly the line of *a, b, c*. There is thus afforded some clue to the determination of the order of the discharges, and I think the order was probably as follows:—VII., VIII., VI., V., IV., I., II., III., the first two occurring during one sweep of the camera; VI., V., IV. during the backward sweep; and I., II., III. in the next onward sweep. There must thus have been an interval of a little over a second between the first and last discharge, for the motion of the camera was at about the rate of three quarters of a second for a complete swing to and fro. The interval between the successive discharges was, therefore, between the fifth and tenth of a second. Had it been much longer the flash would have presented to the eye a flickering appearance.

Whatever be the explanation of some of the effects noticed above, it is evident that a lightning flash has not the simple instantaneous character formerly supposed; but that it consists of a varying number of successive discharges following one another in the same path at intervals which may in some cases be comparatively long.

Note.—Since the above was written I have had an opportunity of inspecting the photographs of banded, ribbon, and curtain lightning collected by the Royal Meteorological Society. There is, I think, no doubt that the explanation of these is afforded by the multiple flashes and luminous streaks noticed above. It is noteworthy that they were always obtained with a camera held either in the hand or in such a way as to render motion probable.

Science Schools, S. Kensington.

XVI. *On the Reflexion and Refraction of Light at the Surface of a Crystal on the Quasi-labile Æther Theory.* By R. T. GLAZEBROOK, M.A., F.R.S.*

IN his comparison of the Electric Theory of Light and the theory of a Quasi-labile Æther, in the Philosophical Magazine for March 1889, Prof. Willard Gibbs has shown that the conditions to be satisfied at the common surface of two media, whether crystalline or not, are the same for the Electromagnetic Theory of Light and for the new Labile Æther theory of Sir William Thomson. The formulæ which give the intensities and azimuths of the planes of polarization for the new theory may therefore be deduced from those for the electromagnetic theory; they may, however, be obtained in a fairly simple manner from the new theory itself, and it seems desirable to have them expressed and so to bring out more clearly the connexion between the two theories†.

The pressural wave in the new theory disappears from the equations of motion within the medium; it has, however, its effect in the conditions at the surface. Let us call u_0, v_0 the components of the displacement in this wave, and let the axis of x be normal to the surface, and the axis of z the intersection of the surface and the plane of the waves. Then for an isotropic medium $u_0 = \frac{d\psi}{dx}, v_0 = \frac{d\psi}{dy}$, where

$$\psi = C \sin \frac{2\pi}{\lambda_0} (l_0 x + m_0 y - V_0 t).$$

Also, if V, l, m, λ are the corresponding quantities for the transverse wave, we have the relations

$$V/\lambda = V_0/\lambda_0, \quad m/\lambda = m_0/\lambda_0; \\ \therefore \psi = C \sin \frac{2\pi}{\lambda} \left\{ \frac{l_0 \lambda}{\lambda_0} x + m y - V t \right\}. \quad . \quad . \quad . \quad (1)$$

Now Sir William Thomson has shown that when $A=0$, and therefore $V_0=0$, we must have $C=0$, but at the same time λ_0 is zero. Thus, if we put $2\pi Cl_0 = D\lambda_0$, we have

* Communicated by the Author.

† Since much of the above was written, a paper on somewhat the same subject has been read before the Mathematical Society by Mr. A. B. Basset. Some of the results of his paper, which is not yet printed, have been communicated to me in a letter from Mr. Basset, and agree with those of the present investigation.—June 18, 1889.

$$\left. \begin{aligned} u_0 &= D \cos \frac{2\pi}{\lambda} \left\{ \frac{l_0 \lambda}{\lambda_0} x + my - Vt \right\} \\ v_0 &= \frac{D \lambda_0}{\lambda} \frac{m}{l_0} \cos \frac{2\pi}{\lambda} \left\{ \frac{l_0 \lambda}{\lambda_0} x + my - Vt \right\} \end{aligned} \right\} \dots (2)$$

Also

$$\frac{du_0}{dy} = \frac{dv_0}{dx} = -\frac{2\pi m}{\lambda} D \sin \frac{2\pi}{\lambda} \left\{ \frac{l_0 \lambda}{\lambda_0} x + my - Vt \right\}, \dots (3)$$

while v_0 vanishes compared with u_0 when λ_0 is zero; and in this case we have merely a surface wave of normal displacement given by u_0 travelling over the boundary.

Let us suppose the same to be the case at the common surface of an isotropic medium and a crystal; we know that the normal wave has no effect in the interior, we shall find shortly that all the surface conditions are satisfied by the hypothesis of a normal displacement over the surface of the same form as at the boundary of two isotropic media.

Let S be the amplitude of the optical disturbance in the incident wave so that the disturbance is

$$S \cos \frac{2\pi}{\lambda} (lx + my - Vt),$$

α, β, γ the angles it makes with the axes, l, m, n the direction cosines of the wave-normal. Let $S_1, \alpha_1, \beta_1, \gamma_1, l_1, m_1, n_1$ refer to the reflected wave; $S', \alpha', \&c.$ to one refracted wave; $S'', \alpha'', \&c.$ to the other. Let χ' and χ'' be the angles between the rays and the wave-normals.

The conditions at the surface are that u, v, w, N_1, T_2, T_3 are all continuous. The last three taken in order give, since we suppose the rigidity the same in all media, and since u, v, w are not functions of z ,

$$\frac{dv}{dy} \frac{du}{dy} + \frac{dv}{dx}, \text{ and } \frac{dw}{dx}$$

continuous. The first of these three is already satisfied by the continuity of v ; and we have thus five equations to find the amplitudes of the two refracted waves, the amplitude and the azimuth of the plane of polarization of the reflected wave, and the amplitude of the surface effect.

These five equations may be written, if we introduce the values of $u_0, \frac{du_0}{dy}$ &c., and divide by the periodic factor, as follows:—

$$S \cos \alpha + S_1 \cos \alpha_1 + D = S' \cos \alpha' + S'' \cos \alpha'' + D' \quad . \quad (4)$$

$$S \cos \beta + S_1 \cos \beta_1 = S' \cos \beta' + S'' \cos \beta'' \quad . \quad (5)$$

$$S \cos \gamma + S_1 \cos \gamma_1 = S' \cos \gamma' + S'' \cos \gamma'' \quad . \quad (6)$$

$$\begin{aligned} & \frac{m \cos \alpha + l \cos \beta}{\lambda} S + \frac{m_1 \cos \alpha_1 + l_1 \cos \beta_1}{\lambda_1} S_1 + \frac{2mD}{\lambda} \\ &= \frac{m' \cos \alpha' + l' \cos \beta'}{\lambda'} S' + \frac{m'' \cos \alpha'' + l'' \cos \beta''}{\lambda''} S'' + \frac{2m'}{\lambda'} D' \quad (7) \end{aligned}$$

$$\frac{Sl \cos \gamma}{\lambda} + \frac{S_1 l_1 \cos \gamma_1}{\lambda_1} = \frac{S' l' \cos \gamma'}{\lambda'} + \frac{S'' l'' \cos \gamma''}{\lambda''}, \quad . \quad (8)$$

with the conditions

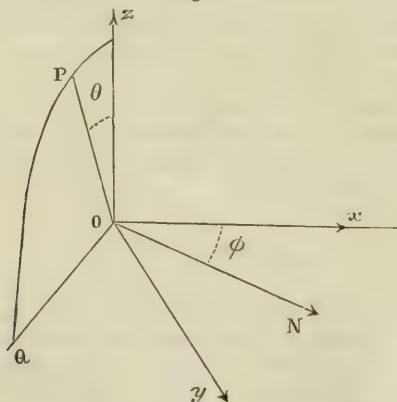
$$\frac{m}{\lambda} = \frac{m_1}{\lambda_1} = \frac{m'}{\lambda'} = \frac{m''}{\lambda''};$$

and on eliminating $D - D'$ from (4) and (7) we find

$$\begin{aligned} & \frac{l \cos \beta - m \cos \alpha}{\lambda} S + \frac{l_1 \cos \beta_1 - m_1 \cos \alpha_1}{\lambda_1} S_1 \\ &= \frac{l' \cos \beta' - m' \cos \alpha'}{\lambda'} S' + \frac{l'' \cos \beta'' - m'' \cos \alpha''}{\lambda''} S''. \quad (9) \end{aligned}$$

This equation, together with (5) (6) and (8), will determine all our unknown quantities. It remains to express them in terms of the angles of incidence and refraction and of the directions of vibration.

Fig. 1.



Let ϕ, ϕ', ϕ'' be the angles of incidence and refraction. Let $\theta, \theta_1, \theta', \theta''$ be the angles between Oz and the directions of vibration, and let $\bar{\theta}', \bar{\theta}''$ be the angles between Oz and the projections on the wave fronts of the directions of vibration (fig. 1).

Let ON be the wave-normal, OP the direction of vibration in the incident wave, and let this wave cut the plane $z=0$ in OQ.

Then clearly

$$\left. \begin{aligned} \cos \alpha &= -\sin \phi \sin \theta \\ \cos \beta &= \cos \phi \sin \theta \\ \cos \gamma &= \cos \theta \end{aligned} \right\} \quad . \quad . \quad . \quad (10)$$

$$\left. \begin{aligned} \cos \alpha_1 &= -\sin \phi \sin \theta_1 \\ \cos \beta_1 &= -\cos \phi \sin \theta_1 \\ \cos \gamma_1 &= \cos \theta_1 \end{aligned} \right\} \quad . \quad . \quad . \quad (11)$$

$$\left. \begin{aligned} l &= \cos \phi, & m &= \sin \phi \\ l_1 &= -\cos \phi, & m_1 &= \sin \phi \\ l' &= \cos \phi', & m' &= \sin \phi' \end{aligned} \right\} \quad . \quad . \quad (12)$$

Again, for the refracted wave, S' is equivalent to $S' \cos \chi'$ in the wave-front, and $S' \sin \chi'$ along the wave-normal. $S' \cos \chi'$ is equivalent to $S' \cos \chi' \cos \bar{\theta}'$ along Oz, and $S' \cos \chi' \sin \bar{\theta}'$ along the intersection of the wave and the plane xy , and this last is equivalent to $S' \cos \chi' \sin \bar{\theta}' \cos \phi'$ along Oy, and $-S' \cos \chi' \sin \bar{\theta}' \sin \phi'$ along Ox. Again, the component $S' \sin \chi'$ along the wave-normal gives $S' \sin \chi' \cos \phi'$ along Ox, and $S' \sin \chi' \sin \phi'$ along Oy.

Hence

$$\left. \begin{aligned} \cos \alpha' &= -\cos \chi' \sin \bar{\theta}' \sin \phi' + \sin \chi' \cos \phi' \\ \cos \beta' &= \cos \chi' \sin \bar{\theta}' \cos \phi' + \sin \chi' \sin \phi' \\ \cos \gamma' &= \cos \chi' \cos \bar{\theta}' \end{aligned} \right\} \quad . \quad . \quad . \quad (13)$$

On substituting these values in equations (6), (8), (9), and (5) respectively we obtain

$$S \cos \theta + S_1 \cos \theta_1 = S' \cos \chi' \cos \bar{\theta}' + \text{terms in } S'' \text{ \&c.} \quad . \quad (14)$$

$$(S \cos \theta - S_1 \cos \theta_1) \cot \phi = S' \cos \chi' \cos \bar{\theta}' \cot \phi' + \text{terms in } S'' \quad . \quad . \quad . \quad (15)$$

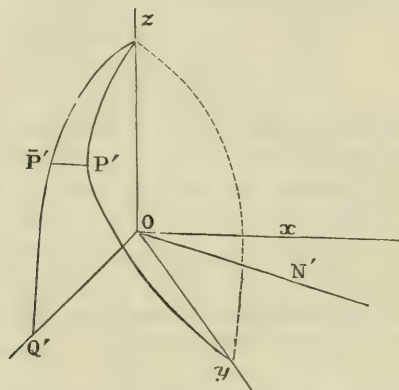
$$(S \sin \theta + S_1 \sin \theta_1) \operatorname{cosec} \phi = S' \cos \chi' \sin \bar{\theta}' \operatorname{cosec} \phi' + \text{terms in } S'' \quad . \quad (16)$$

$$(S \sin \theta - S_1 \sin \theta_1) \cos \phi = S' \cos \chi' (\sin \bar{\theta}' \cos \phi' + \sin \phi' \tan \chi') + \text{terms in } S'' \quad . \quad (17)$$

The corresponding equations on the electromagnetic theory are given in the same form in a paper by myself in the 'Proceedings of the Cambridge Philosophical Society' (vol. iv. p. 165, equations 24-27). If we suppose the magnetic permeability the same in the two media, and write Σ for the

amplitude of the electric displacement, then the two sets of equations are identical, provided $S \cos \chi$ is proportional to $V^2 \Sigma$, V being the velocity of light and χ the angle between the ray and the wave-normal; that is to say, provided that the electric displacement is proportional to the component in the wave-front of the actual displacement, and inversely proportional to the square of the velocity of light. It must be remembered that θ' does not determine the direction of vibration in the refracted wave, but the projection of that direction on the wave-front.

Fig. 2.



Again, let ψ' be the angle between the plane through Oz and the direction of vibration and the plane of the refracted wave; then we have, if OP' (fig. 2) be the direction of vibration, $O\bar{P}'$ its projection on the wave-front,

$$\begin{aligned} zP' &= \theta', & z\bar{P}' &= \bar{\theta}', & P'\bar{P}' &= \chi', \\ P'z\bar{P}' &= \psi', & yz\bar{P}' &= \phi', & yP' &= \beta', \\ \therefore P'zy &= \phi' - \psi'; \end{aligned}$$

and we readily find that

$$\begin{aligned} \cos \beta' &= \sin \theta' \cos (\phi' - \psi'), \\ \cos \theta' &= \cos \bar{\theta}' \cos \chi', \\ \sin \bar{\theta}' \cos \chi' &= \sin \theta' \cos \psi'. \end{aligned}$$

So that the last two equations (16) and (17) become

$$(S \sin \theta + S_1 \sin \theta_1) \operatorname{cosec} \phi = S' \sin \theta' \cos \psi' \operatorname{cosec} \phi' + \text{terms in } S'', \quad (18)$$

$$(S \sin \theta - S_1 \sin \theta_1) \cos \phi = S' \sin \theta' \cos \psi' \cos (\phi' - \psi') + \text{terms in } S'', \quad (19)$$

forms which may sometimes be useful.

If we take the case in which only one wave traverses the crystal we find the following relations by eliminating the ratios S/S_1 and S/S' :—

$$\tan \theta = \tan \theta' \cos (\phi - \phi') + \frac{\sin^2 \phi' \tan \chi'}{\cos \theta' \sin (\phi + \phi')}, \quad \dots \quad (20)$$

$$\tan \theta_1 = -\tan \theta \frac{\cos (\phi + \phi')}{\cos (\phi - \phi')} + \frac{2 \sin 2\phi \sin^2 \phi' \tan \chi'}{\sin 2(\phi - \phi') \sin (\phi + \phi') \cos \theta'} \\ \dots \dots \dots (21)$$

The first of these equations was tested by me experimentally, the second medium being Iceland spar (see Phil. Trans. 1882, Part II.). There was fair agreement between the theory and experiment; but the errors of the experiment were larger than they need have been, in consequence of some want of annealing in one of the lenses of the telescope used in the observations, which was not discovered till too late (Proc. R. S. vol. xxxiv. p. 233). Equations the same as (20) and (21) have been obtained by Neumann, MacCullagh, Kirchhoff, and others.

Equation (4) gives us a value for $D' - D$. We find

$$\begin{aligned} D - D' &= \sin \phi (S \sin \theta + S_1 \sin \theta_1) \\ &\quad - S' \cos \chi' (\sin \bar{\theta}' \sin \phi' - \tan \chi' \cos \phi') - \text{terms in } S'' \\ &= S' \cos \chi' \sin \theta' \operatorname{cosec} \bar{\phi}' (\sin^2 \phi - \sin^2 \phi') \\ &\quad + S' \sin \chi' \cos \phi' + \text{terms in } S'' \text{ \&c.} \\ &= S' \cos \chi' \operatorname{cosec} \phi' \{ \sin (\phi + \phi') \sin (\phi - \phi') \sin \bar{\theta}' \\ &\quad + \tan \chi' \cos \phi' \sin \phi' \} + \text{terms in } S'' \text{ \&c.} \quad (22) \end{aligned}$$

Thus the problem of reflexion and refraction at a crystal on the Labile Æther theory is fully solved, and some of the results exhibited in a form which can be tested by experiment, though the experimental results will not discriminate between it and the electric theory.

It will be noticed that the terms in D and D' arising from the surface action have no place in the electric problem. Equation (26) of my paper in the C. P. S. Proceedings, already referred to, which is the same as equation (16) above, expresses the conditions either that the electric displacement along the normal, or that the magnetic force along the axis of z is the same in the two media. So long as we suppose the surface to remain unelectrified, these conditions lead to the same equations. On the Labile Æther theory the two conditions

of equality of normal displacement and of surface traction parallel to Oz cannot be satisfied without some surface action. The elimination of the terms expressing this surface-action from these two equations of condition gives us our equation (16). On the Electric theory, if we suppose a surface distribution of variable density possible, terms would come into the two surface conditions already mentioned, depending on this distribution; we should thus have two equations corresponding to our (4) and (7), and the elimination of the surface-density from these would give us an equation equivalent to (16).

It is perhaps worth while to remark that equation (9) or (16) holds, even though the constant A be not zero. For since u is continuous across the surface, so is also $\frac{du}{dy}$; and since $\frac{dv}{dx} + \frac{du}{dy}$ is continuous, we see that $\frac{dv}{dx} - \frac{du}{dy}$ is also continuous. But we have $\frac{dv_0}{dx} = \frac{du_0}{dy}$; and hence, in the expression for the continuity of $\frac{dv}{dx} - \frac{du}{dy}$, the terms involving the pressural wave will not occur, and this condition will give us equation (9) at once. But if A is not zero, (5) will be modified, and becomes

$$S \cos \beta + S_1 \cos \beta_1 + \frac{D\lambda_0 m}{\lambda l_0} = S' \cos \beta' + S'' \cos \beta'' + \frac{D'\lambda_0' m'}{\lambda' l_0'}; \quad (23)$$

while the continuity of N_1 leads, if we assume A to be the same in both media, to

$$A \left(\frac{du}{dx} + \frac{dv}{dy} \right) - 2B \frac{dv}{dy} = A \left(\frac{du'}{dx} + \frac{dv'}{dy} \right) - 2B \frac{dv'}{dy}; \quad (24)$$

which since v , and therefore $\frac{dv}{dy}$, is continuous, reduces to

$$\frac{du}{dx} = \frac{du'}{dx'} \quad . \quad . \quad . \quad . \quad . \quad . \quad (25)$$

We also require the equation of motion for the pressural wave, and the problem is much more complicated; it has been solved for two isotropic media in Sir W. Thomson's paper, and the solution in the present case must proceed along the same lines.

XVII. *On the Propagation of Electric Waves through Wires.*
By Prof. H. HERTZ.*

IF a constant electric current flows in a cylindrical wire, its intensity is the same in all parts of the section of the wire. But if the current is variable, self-induction causes a deviation from this most simple distribution. For, since the inner parts of the wire are in the mean less distant from all the rest than are those on the circumference, induction opposes alterations of the current in the interior of the wire more strongly than at the circumference; and in consequence of this the flow is confined to the exterior of the wire. If the current alters its direction a few hundred times per second the deviation from the normal distribution is no longer imperceptible; this deviation increases rapidly with the rate of alternation, and when the current alternates many millions of times per second, according to theory almost the whole interior of the wire must appear free from current, and the flow must be confined to the immediate neighbourhood of the circumference. In such very extreme cases the hitherto accepted theory of the phenomenon is plainly not without physical difficulties; and preference must be given to another view of the subject, which was indeed first put forward by Messrs. Heaviside† and Poynting‡ as the true interpretation of the equations of Maxwell as applied to this case. According to this view, the electric force which determines the current is in no wise propagated in the wire itself, but under all circumstances enters the wire from without and spreads itself in the metal comparatively slowly, and according to similar laws as changes of temperature in a conductor of heat. If the forces in the neighbourhood of the wire are continually altering in direction, the effect of these forces will only enter to a small depth into the metal; the more slowly the changes take place, so much deeper will the effect penetrate; and if, finally, the changes follow one another infinitely slowly, the force has time to fill the whole interior of the wire with uniform intensity.

In whatever way we wish to regard the results of the theory, an important question is, whether it agrees with fact. Since, in the experiments which I carried out on the propagation of electric force, I made use of electric waves in wires which

* Translated from Wied. *Ann.* xxxvii. p. 395 (July 1889), by Dr. J. L. Howard, Demonstrator of Physics in University College, Liverpool.

† Heaviside, *Electrician*, Jan. 1885; *Phil. Mag.* [5] xxv. p. 153 (1888).

‡ Poynting, *Phil. Trans.* ii. p. 277 (1885).

were of extraordinarily short period, it was convenient to prove by means of these the accuracy of the inferences drawn. In fact, the theory was proved by the experiments which will now be described; and it will be found that these few experiments suffice to confirm in the highest degree the view of Messrs. Heaviside and Poynting. Analogous experiments, with similar results, but with quite different apparatus, have already been made by Dr. O. J. Lodge*, chiefly in the interest of the theory of lightning-conductors. Up to what point the conclusions are just which were drawn by Dr. Lodge in this direction from his experiments, must depend in the first place on the velocity with which the alterations of the electrical conditions really follow each other in the case of lightning.

The apparatus and methods which are here mentioned are those which I have described in full in previous memoirs†. The waves used were such as had in wires a distance of nearly 3 metres between the nodes.

1. If a primary conductor acts through space upon a secondary conductor, it cannot be doubted that the effect penetrates the latter from without. For it can be regarded as established that the effect is propagated in space from point to point, therefore it will be forced to meet first of all the outer boundary of the body before it can act upon the interior of it. But now a closed metallic envelope is shown to be quite opaque to this effect. If we place the secondary conductor in such a favourable position near the primary one that we obtain sparks 5 to 6 mm. long, and surround it now with a closed box made of zinc plate, the smallest trace of sparking can no longer be perceived. The sparks similarly vanish if we entirely surround the primary conductor with a metallic box. It is well known that, with relatively slow variations of current, the integral force of induction is in no way altered by a metallic screen. This is, at the first glance, contradictory to the present experiments. However, the contradiction is only an apparent one, and is explained by considering the duration of the effects. In a similar manner, a screen which conducts heat badly protects its interior completely from rapid changes of the outside temperature, less from slow changes, and not at all from a continuous raising or lowering of the temperature. The thinner the screen is the more rapid are the variations of the outside temperature which can be felt in its interior. In our case also the electrical action must plainly penetrate into the interior, if

* Lodge, Journ. Soc. Arts, May 1888; Phil. Mag. [5] xxvi. p. 217 (1888).

† Hertz, Wied. Ann. xxxiv. p. 551 (1888).

we only diminish sufficiently the thickness of the metal. But I did not succeed in attaining the necessary thinness in a simple manner ; a box covered with tinfoil protected completely, and even a box of gilt paper, if care was taken that the edges of the separate pieces of paper were in metallic contact. In this case the thickness of the conducting-metal was estimated to be barely $\frac{1}{20}$ mm. I now fitted the protecting envelope as closely as possible round the secondary conductor. For this purpose its spark-gap was widened to about 20 mm., and in order to detect electrical disturbances in it an auxiliary spark-gap was added exactly opposite the one ordinarily used. The sparks in this latter were not so long as in the ordinary spark-gap, since the effect of resonance was now wanting, but they were still very brilliant. After this preparation the conductor was completely enclosed in a tubular conducting envelope as thin as possible, which did not touch it, but was as near it as possible ; and in the neighbourhood of the auxiliary spark-gap (in order to be able to use it) the envelope contained a wire-gauze window. Between the poles of this envelope brilliant sparks were produced, just as previously in the secondary conductor itself ; but in the enclosed conductor not the slightest electrical movement could be recognized. The result of the experiment is not affected if the envelope touches the conductor at a few points ; the insulation of the two from each other is not necessary in order to make the experiment succeed, but only to give it the force of a proof. Clearly we can imagine the envelope to be drawn more closely round the conductor than is possible in the experiment ; indeed, we can make it coincide with the outermost layer of the conductor. Although, then, the electrical disturbances on the surface of our conductor are so powerful that they give sparks 5 to 6 mm. long, yet at $\frac{1}{20}$ mm. beneath the surface there exists such perfect freedom from disturbance that it is not possible to obtain the smallest sparks. We are brought, therefore, to the conclusion that what we call an induced current in the secondary conductor is a phenomenon which is manifested in its neighbourhood but to which its interior scarcely contributes.

2. One might grant that this is the state of affairs when the electric disturbance is conveyed through a dielectric, but maintain that it is another thing if the disturbance, as one usually says, has been propagated in a conductor. Let us place near one of the end plates of our primary conductor a conducting-plate, and fasten to it a long straight wire ; we have already seen in the previous experiments how the effect of the primary oscillation can be conveyed to great distances

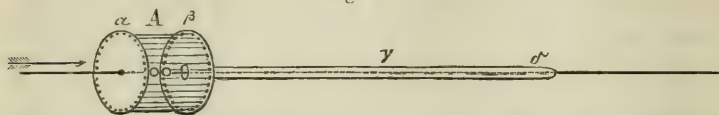
by the help of this wire. The usual theory is that a wave travels along the wire in this case. But we shall try to show that all the alterations are confined to the space outside and the surface of the wire, and that its interior knows nothing of the wave passing over it. I arranged experiments first of all in the following manner. A piece about 4 metres long was removed from the wire conductor and replaced by two strips of zinc plate 4 metres long and 10 cm. broad, which were laid flat one above the other, with their ends permanently connected together. Between the strips along their middle line, and therefore almost entirely surrounded by their metal, was laid along the whole 4 metres length a copper wire covered with gutta-percha. It was immaterial for the experiments whether the outer ends of this wire were in metallic connexion with, or insulated from, the strips; however, the ends were mostly soldered to the zinc strips. The copper wire was cut through in the middle, and its ends were carried, twisted round each other, outside the space between the strips to a fine spark-gap, which permitted the detection of any electrical disturbance taking place in the wire. When waves of the greatest possible intensity were sent through the whole arrangement, there was nevertheless not the slightest effect observable in the spark-gap. But if the copper wire was then displaced anywhere a few decimetres from its position, so that it projected just a little beyond the space between the strips, sparks immediately began to pass. The sparks were the more intense according to the length of copper wire extending beyond the edge of the zinc strips and the distance it projected. The unfavourable relation of the resistances was therefore not the cause of the previous absence of sparking, for this relation has not been changed; but the wire being in the interior of the conducting mass, was at first deprived of the influence coming from without. Moreover, it is only necessary for us to surround the projecting part of the wire with a little tinfoil in metallic communication with the zinc strips, in order to immediately stop the sparking again. By this means we have brought the copper wire back again into the interior of the conductor. If we bend another wire into a fairly large arc round the projecting portion of the gutta-percha wire, the sparks will be likewise weakened; the second wire takes off from the first a certain amount of the effect due to the outer medium. Indeed, it may be said that the edge of the zinc strip itself takes away the induction from the middle of the strip in a similar manner. For if we now remove one of the strips, and leave the insulated wire simply resting on the other one, we certainly obtain sparks continuously in the wire; but they are extremely weak

if the wire lies along the middle of the strip, and much stronger when near its edge. Just as in the case of distribution under electrostatic influence the electricity would prefer to collect on the sharp edge of the strip, so also here the current tends to move along the edge. Here, as there, it may be said that the outermost parts screen the interior from outside influence.

The following experiments are somewhat neater and equally convincing. I inserted into the conductor transmitting the waves a very thick copper wire, 1.5 metre long, whose ends carried two circular metallic disks of 15 cm. diameter. The wire passed through the centres of the disks; the planes of the disks were at right angles to the wire; each of them had on its rim 24 holes, at equal distances apart. A spark-gap was inserted in the wire. When the waves traversed the wire they gave rise to sparks as much as 6 mm. long. A thin copper wire was then stretched across between two corresponding holes of the disks. On doing this, the length of the sparks sank to 3.2 mm. There was no further alteration if a thick copper wire was put in the place of the thin one, or if, instead of the single thin wire, twenty-four of them were taken, provided they were placed near each other through the same two holes. But it was otherwise if the wires were distributed over the rim of the disks. If a second wire was inserted opposite the first one, the spark-length fell to 1.2 mm. When two more wires were added midway between the first two, the length of the spark sank to 0.5 mm.; the insertion of four more wires still in the mean positions left sparks of scarcely 0.1 mm. long; and after inserting all the twenty-four wires at equal distances apart, not a trace of sparking was perceptible in the interior. The resistance of the inner wire was nevertheless much smaller than that of all the outside wires taken together; we have also a still further proof that the effect does not depend upon this resistance. If we place by the side of the partial tube of wires, and in parallel circuit with them, a conductor in all respects similar to that in the interior of the tube, we have in the former brilliant sparks, but none whatever in the latter. The former is unprotected, the latter is screened by the tube of wires. We have in this an electrodynamic analogue of the electrostatic experiment known as the electric birdcage. I again altered the experiment, in the manner depicted in fig. 1, p. 122. The two disks were placed so near together that they formed, with the wires inserted between them, a cage (A) just large enough for the reception of the spark-micrometer. One of the disks, α , remained metallically connected with the central wire; the other, β , was insulated from the wire by means of a circular hole through its centre,

at which it was connected to a conducting-tube, γ , which, insulated from the central wire, surrounded it completely for

Fig. 1.



a length of 1.5 metre. The free end of the tube, δ , was then connected with the central wire. The wire, together with its spark-gap, is once more situated in a metallicallly protected space; and it was only to be expected, from the previous experiments, that not the slightest electrical disturbance would be detected in the wire in whichever direction waves were sent through the apparatus. So far, then, this arrangement promises nothing new, but it has the advantage over the previous one that we can replace the protecting metallic tube, γ , by tubes of smaller and smaller thickness of wall, in order to investigate what thickness is still sufficient to screen off the outside influence. Very thin brass tubes, tubes of tinfoil and Dutch metal proved to be perfect screens. I now took glass tubes which had been silvered by a chemical method, and it was then perfectly easy to insert tubes of such thinness that, in spite of their protecting power, brilliant sparks occurred in the central wire. But sparks were only observed when the silver film was no longer quite opaque to light and was certainly thinner than $\frac{1}{100}$ mm. In imagination, although not in reality, we can conceive the film drawn closer and closer round the wire, and finally coinciding with its surface; we should be quite certain that nothing would be radically altered thereby. However actively, then, the real waves play round the wire, its interior remains completely at rest; and the effect of the waves hardly penetrates any more deeply into the interior of the wire than does the light which is reflected from its surface. For the real seat of these waves we ought not to look, therefore, in the wire, but rather to assume that they take place in its neighbourhood; and instead of asserting that our waves are propagated in the wire, we should be more accurate in saying that they glide along on the wire.

Instead of placing the apparatus just described in the circuit in which we produced waves indirectly, we can insert it in one branch of the primary conductor itself. In such experiments I obtained results similar to the previous ones. Our primary oscillation, therefore, takes place without any participation of the conductor in which it is excited, except at its

bounding surface ; and we ought not to look for its existence in the interior of the conductor*.

To what has been said above about waves in wires we wish to add just one remark concerning the method of carrying out the experiments. If our waves have their seat in the neighbourhood of the wire, the wave progressing along a single isolated wire will not be propagated through the air alone ; but since its effect extends to a great distance it will partly be transmitted by the walls, the ground, &c., and will thus give rise to a complicated phenomenon. But if we place opposite each pole of our primary conductor in exactly the same way two auxiliary plates, and attach a wire to each of them, carrying the wires straight and parallel to each other to equal distances, the effect of the waves makes itself felt only in the region of space between the two wires. The wave progresses solely in the space between the wires. We can thus take precautions to propagate the effect through the air alone or through another insulator, and the experiments will be more convenient and free from error by this arrangement. For the rest, the lengths of the waves are nearly the same in this case as in isolated wires, so that with the latter the effect of the disturbing causes is apparently not considerable.

3. We can conclude from the above results that rapid electric oscillations are quite unable to penetrate metallic sheets of any thickness, and that it is, therefore, impossible by any means to excite sparks by the aid of such oscillations in the interior of closed metallic screens. If, then, we see sparks produced by such oscillations in the interior of metallic conductors, which are nearly, but not quite, closed, we shall be obliged to conclude that the electric disturbance has forced itself in through the existing openings. This view is also correct, but it contradicts the usual theory in some cases so completely that one is only induced by special experiments to give up the old theory in favour of the new one. We shall choose a prominent case of this kind, and by assuring ourselves of the truth of our theory in this case, we shall demonstrate its probability in all other cases. We again take the arrangement which we have described in the previous section and drawn in fig. 1 ; only we now leave the protecting tube insulated from the central wire at δ . Let us now send a series of waves through the apparatus in the direction

* The calculation of the self-induction of such conductors on the assumption of uniform density of current in their interior must therefore lead to quite erroneous results. It is to be wondered at that the results obtained with such wrong assumptions should still appear to approximately coincide with truth.

from A towards δ . We thus obtain brilliant sparks at A ; they are of similar intensity to those obtained when the wire was inserted without any screen. The sparks do not become materially smaller, if, without making any other alteration, we lengthen the tube γ considerably, even to 4 metres. According to the usual theory it would be said that the wave arriving at A penetrates easily the thin, good-conducting metal disk α , then it leaps across the spark-gap at A, and travels on in the central wire. According to our view, on the contrary, we must explain the phenomenon in the following manner. The wave arriving at A is quite unable to penetrate the metallic disk ; it therefore glides along the disk over the outside of the apparatus and travels as far as the point δ , 4 metres away. Here it divides : one part, which does not concern us at present, travels on immediately along the straight wire, another bends into the interior of the tube and then runs back in the space between the tube and the central wire to the spark-gap at A, where it now gives rise to the sparking. That our view, although more complicated, is still the correct one, is proved by the following experiments. Firstly, every trace of sparking at A disappears as soon as we close the opening at δ , even if it be only by a stopper of tinfoil. Our waves have only a wave-length of 3 metres ; before their effect has reached the point δ the effect at A has passed through zero and changed sign. What influence then could the closing of the distant end δ have upon the spark at A, if the latter really happened immediately after the passage of the wave through the metallic wall ? Secondly, the sparks disappear if we make the central wire terminate inside the tube γ , or at the opening δ itself ; but they reappear when we allow the end of the wire to project even 20 to 30 centim. only beyond the opening. What influence could this insignificant lengthening of the wire have upon the sparks in A, unless the projecting end were just the means by which a part of the wave breaks off and penetrates through the opening δ back into the interior ? Thirdly, we insert in the central wire between A and δ a second spark-gap B, which we also completely cover with a gauze cage like that at A. If we make the distance of the terminals at B so great that sparks can no longer pass across, it is also no longer possible to obtain visible sparks at A. But if we hinder in like manner the passage of the spark at A, this has scarcely any influence on the sparks in B. Therefore, the passage of the spark at B determines that at A, but the passage of a spark at A does not determine that at B. The direction of propagation in the interior is therefore from B towards A, not from A to B.

We can moreover give further proofs, which are more convincing. We may prevent the wave returning from δ to A from dissipating its energy in sparks, by making the spark-gap either vanishingly small or very great. In this case the wave will be reflected at A, and will now return again from A towards δ . In doing so, it must meet the direct waves from δ to A and combine with them to form stationary waves, thus giving rise to nodes and ventral segments. If we succeed in proving their existence, there will be no longer any doubt as to the truth of our theory. For this proof we must give somewhat different dimensions to our apparatus in order to be able to introduce electric resonators into its interior. I therefore led the central wire through the axis of a cylindrical tube 5 metres long and 30 centim. diameter. It was not constructed of solid metal, but of 24 wires arranged parallel to each other along the generating surface, and resting on seven equidistant and circular rings of strong wire, as shown in fig. 2. I made the requisite resonator in the following manner:—A closely-wound spiral of 1 centim. diameter was

Fig. 2.



formed from copper wire of 1 millim. thickness; about 125 turns of this spiral were taken, drawn out a little, and bent into a circle of 12 centim. diameter; between the free ends an adjustable spark-gap was inserted. Previous experiments had shown that this circle responded to waves 3 metres long in the wire, and yet it was small enough in size to admit of its insertion between the central wire and the surface of the tube. If now both ends of the tube were open, and the resonator was then held in the interior in such a way that its plane included the central wire, and its spark-gap was not directed exactly inwards or outwards, but was turned towards one end or the other of the tube, brilliant sparks of $\frac{1}{2}$ to 1 millim. length were observed. On now closing both ends of the tube by four wires arranged crosswise and connected with the central conductor, not the slightest sparking remained in the interior, a proof that the network of the tube is a sufficiently good screen for our experiments. The end of the tube on the side β , that, namely, which was furthest away from the origin of the waves, was now removed. In the immediate neighbourhood of the closed end, that is at the point α which corresponds to the spark-gap A of our previous experiments, there were now no sparks observable in the resonator. But on

moving away from this position towards β , sparks appeared, became very brilliant at a distance of 1.5 metre from α , then decreased again in intensity, they almost entirely vanished at 3 metres distance from α , and increased again until the end of the tube was reached. We thus find our theory borne out by fact. That we obtain a node at the closed end is clear, for at the metallic contact between the central wire and the surface of the tube the electric force between the two must necessarily vanish. It is different when we cut the central conductor at this point just near the end, and insert a gap of several centimetres length. In this case the wave will be reflected in a phase opposite to that of the previous case, and we should expect a ventral segment at α . As a matter of fact we find brilliant sparks in the resonator in this case; and they rapidly decrease in strength if we move from α towards β , they almost entirely vanish at a distance of 1.5 metre, and become brilliant again at a distance of 3 metres; moreover they give a second well-marked node at 4.5 metres distance, that is 0.5 metre from the open end. The nodes and loops which we have described are situated at fixed distances from the closed end, and alter only with this distance; they are, however, quite independent of the occurrences outside the tube, for example, of the nodes and loops formed there. The phenomena occur in exactly the same way if we allow the wave to travel through the apparatus in the direction from the open to the closed end; their interest is, however, smaller, since the mode of transmission of the wave deviates from that usually conceived, less in this case than in the one which has just been under our consideration. If both ends of the tube are left open with the central wire undivided, and stationary waves with nodes and loops are now set up in the whole system, there is always found, for every node outside the tube, a corresponding node in the interior; which proves that the propagation takes place inside and outside with, at any rate approximately, the same velocity.

On looking over the experiments which we have described, and the interpretation put upon them, as well as the explanations of the physicists referred to in the introduction, a difference will be noticed between the views here put forward and the usual theory. According to the latter, conductors are represented as those bodies which alone take part in the propagation of electric disturbances; non-conductors are the bodies which oppose this propagation. According to our view, on the contrary, all transmission of electrical disturbances is brought about by non-conductors: conductors oppose a great resistance to any rapid changes in this transmission. One might almost be inclined to maintain that conductors

and non-conductors should, on this theory, have their names interchanged. However, such a paradox only arises because one does not specify the kind of conduction or non-conduction considered. Undoubtedly metals are non-conductors of electric force, and just for this reason they compel it under certain circumstances to remain concentrated instead of becoming dissipated, and thus they become conductors of the apparent source of these forces, electricity, to which the usual terminology has reference.

Karlsruhe, March 1889.

XVIII. *An Apparatus Illustrating Crystal Forms.* By R. J. ANDERSON, M.A., M.D., *Professor of Natural History in Queen's College, Galway**.

[Plate II.]

THE apparatus by which I propose to illustrate crystal forms consist of frameworks and cords and weights.

The first piece of apparatus is figured in Plate II. fig. 1, and consists of a frame made of wood. This is divided into two compartments. One of these has, above, a slit half an inch wide that runs from end to end; in this slit a slide moves to and fro, and can be fixed by means of a binding-screw at any place.

A slide of a similar kind moves in a slit in the lower part of the framework; this can be fixed by binding-screws in any position. Pulleys are fixed at the ends of this compartment.

Slips of wood run from end to end at the sides and carry riding-slides. These slides have binding-screws and pulleys whose sheaves revolve on a vertical axis fitted to them.

A figure is easily constructed by carrying cords over the pulleys. Single cords only are shown in the figure. This is for the sake of distinctness.

Starting from α' , which marks a ring connected with the weight p , a single cord runs through β' (ring), η' (ring), δ' , through ring α' , through η' , to be fixed to a weight.

A second cord starting from γ' runs through α' , ϵ' , η' , through ring γ' , and then across to ϵ' through this ring to hook up a weight p' . A third cord is fixed to δ' and runs through ϵ' , β' , γ' , through δ' and β' to loop up another weight. The actual tension-weights are fixed to the small rings, which act as pulleys.

Each rhombus has in this way a cord to itself, and the size of the rhombus depends on the weights attached. The smaller the rhombus the more cord is to spare.

The figure shown is the regular octahedron if the axes be

* Communicated by the Physical Society: read April 13, 1889.

equal. This condition is easily produced by adjusting the weights.

The octahedron of the second dimetric system, or pyramidal system, is produced by increasing the weights above and below.

The octahedron of the third system may be easily formed by increasing a pair of the horizontal weights.

The octahedral figures may be easily formed by leaving out the diagonals and running the cords from the rings at one extremity of the rhombuses through two rings, and then through the opposite ring, to be there fixed to a weight. The tension-weights, as shown in the figure, will then correspond to the apices of the rhombuses.

For the oblique systems further changes are necessary. The upper slide is moved to the right and the lower to the left, or *vice versa*. This is attended with elongation of the vertical axes, and the cords passing through the pulleys above and below and at the ends are increased, and the slack below is pulled in to a less extent. The other sides of the octahedron are less affected.

In the first place, the lateral rider-slides are allowed to remain in a position such that the line joining them is perpendicular to the central vertical longitudinal plane. This gives the Monoclinic System.

Secondly, the rider-slides are moved one to the right, the other to the left, and in this way the *Anorthic* or Triclinic System is produced.

In each case it is desirable to have the slack for each rhombus at different angles of the octahedron.

All the possible varieties of the fifth system cannot be produced in this way. So it is necessary to arrange for the elevation and depression to the rider-slides in extreme cases. This is accomplished by means of a large ring which carries a pulley.

I have chosen the octahedron as the simplest figure.

The cube is formed by the introduction of two horizontal hoops, one above and one below the level of the horizontal bars. These by a simple mechanism are made movable; and if eight pulleys be fixed opposite the eight edges of the octahedron, and the edges of the octahedron be drawn out by rings running on these cords, it will be necessary, then, only to run cords through rings above and below, and to relax the horizontal and apical weights in order to produce the cube.

The modifications caused by truncating or bevelling the edges or faces can be produced by increasing the number of the hoops or rings. For the simplest figures, however, vertical hoops answer best. The sliding-rings that are carried by the

cords may with their transverse cords be lowered to the level of the bar again, and the octahedron again produced.

The cube and the corresponding forms of the pyramidal and prismatic systems may be easily constructed by running the cords as follows:—

Take the cube as

$$\begin{array}{cccc} \alpha' & b' & c' & d' \\ e' & f' & g' & h' \end{array}$$

where the first row represents the upper face, and the second the lower, as in fig. 2.

The cords will have the following course:—

$$\begin{array}{cccccc} b' & g' & h' & f' & b' & a' \\ e' & d' & c' & f' & e' & b' \\ f' & a' & d' & g' & f' & c' \\ c' & h' & e' & b' & c' & h' \\ & & & & & g' \\ & & & & & c' \\ & & & & & a' \\ & & & & & e' \end{array}$$

The faces of the cube corresponding to the angle of the octahedron.

Now by drawing out the cords opposite the middle of the faces (that is, the diagonals of the faces) a 24-sided figure is produced which can be reduced in the limit to a 12-sided figure, namely the rhombic dodecahedron.

If the sides be connected by cords with pulleys and drawn out, and at the same time cords connecting the centres of the sides with the centres of the faces be drawn out, then the trapezohedron is produced.

Cause the two lateral pulleys of the cube to approach above and the longitudinal pair to approach below, and the tetrahedron is produced. A prism surmounted by pyramids is produced by drawing out the diagonals of the terminal faces; from this the corresponding octahedron may be obtained by a simple method.

The other part of the framework is shown in the drawing (fig. 1, left-hand) as containing the double hexagonal pyramid. Sliding-pulleys, as in the part already described, are fitted above and below. Rider-pulleys are attached to the bars at the sides. Two are shown on each side.

The cords are attached in this way:—A bundle of six are fastened together above to a cord, and drawn by this cord through a ring. The cord passes over two pulleys and reaches a weight outside the framework. The six cords pass through the rings marked β , γ , δ , ξ , θ , and ϵ in the figure, then down to be attached to a cord below, which goes through a pulley-sheaf. The rings marked by the Greek letters are seen attached by cords to weights, through each of these a cord passes. This cord is carried through one of the rings and

kept there by means of a small weight. The ratio of the vertical to the horizontal axes may be easily altered by means of the weights. The approximation of the lateral pulley gives rise to the octahedron. The number of the sides may be increased by increasing the cords and pulleys.

In order to show other figures two hoops are fitted to the framework, above and below. The cords of the pyramids are hooked out, and the cords connected with the hooks pass over pulleys and are attached to weights. A cord is made to go through the rings (hook-rings) above and below. By running down the rings and unhooking the weights above and below, the hexagonal prism is produced.

Prisms with more sides can be produced by increasing the number of the cords, which correspond to the edges. The pyramids surmounting the prism are produced by drawing out the cords at the extremities of the prism. Figures with fewer sides are produced by causing the pulleys to approach. Forms the result of bevelling and truncation are produced by pulling out the cords of the terminal pyramids and running other cords through the rings. The original double six-sided prisms are produced by causing the hoops to approach one another.

The ikosahedron is produced by forming the five-faced equilateral pyramid above and below, and approaching the hoops towards one another, so that the distance between the hoops is equal to the perpendicular of one of the triangles. Then it is only necessary to rotate the lower hoop through 36° , and to connect the obtuse angles of the rhombus. In this way the figure can be produced.

The relations of the hexagonal to the rhombohedral division of the sixth system may be shown in this way. Take the double pyramid, hook up each alternate horizontal angle, and hook down the others. Adjust suitably the superior and inferior angles, and the rhombohedron is produced. The cords in reality follow the course of the lines in the glass models.

This method is very interesting in this way, that by a little dodging the rhombohedron can be converted into the cube, so that the relations between the sixth and other systems are rendered more distinctly apparent.

The rhombohedron may be easily changed into the hexahedron by unhooking the weights and pulling in the cord. The hoops are shown in the lower part of fig. 3, Pl. II., with the rhombohedron attached. The hexagonal prism is figured separately for the sake of distinctness.

The ikosahedron may be produced by hooking up and down the horizontal cords of the decahedral pyramids. If

we begin with the double octahedral pyramids, the rhombic dodecahedron can be easily produced by hooking up the corresponding alternate edges above and below, and running cords through the hooks looped up and those rings still stationary.

In order to show the effects of uniting and separating forces the form shown at fig. 2 is useful. The instrument consists of a frame in which hoops revolve, some on vertical and others on horizontal axes. The hoops carry sliding-pulleys as shown on the plate. The cube is easily constructed by running cords over eight pulleys fixed on two rings revolving on a vertical axis. Cords are carried through small rings above and below (fig. 2, $a' b' c' d' e' f' g' h'$).

Without going into details, it will be easily seen that one orthogonal hexahedron can be easily changed into another, and into the corresponding octahedron. The octahedron of the first system, $abcdef$, if constructed by running cord over the pulley B, and the pulley attached to the same ring below, may be changed into the octahedron of the dimetric or trimetric system, or of either of the oblique. The latter is accomplished by causing the hoop to revolve, and for the triclinic the vertical hoops come into action. Adjustment of the weights leads to an alteration in the axes, and the relations of the weights for a special form may be studied.

It is evident that the dodecahedron and trapezohedron may be produced in this instrument as in the first, and that the forms due to truncating or bevelling of the sides are obtained very readily.

The following are the advantages of the apparatus:—First, it shows clearly the effect of changes of force in producing changes of form. The weights can be approximated or separated, and thus the relations of allied forms may be studied. The number of weights may be increased, and the change of form by grouping may in this way be well shown.

If we take an india-rubber tissue ball inflated with air as an example of an infinite number of forces acting from a centre, and a piece of stretched cord with a weight attached as the other extreme limit, many of the intermediate conditions where strings are made to form the edges of figures may be easily understood from the arrangements I have described.

It is true that such methods as are here suggested are open to the objection that mathematical principles of a very important kind are involved. I think the same objection may be made to any mechanical contrivance; but so far from getting rid of a difficulty without explaining it, I hold that the apparatus, whilst it will produce a better conception of crystal forms, and the actual work in the crystals themselves,

in the minds of those students who know very little about mathematics, viz., almost all students of chemistry and mineralogy, and a still larger number of geological students, the apparatus will prove useful to mathematical students inasmuch as the arithmetical processes are tedious and complex for even those forms in which the mathematical relations are comparatively simple. For the forms with oblique axes the advantages of a simple method of noting the weights necessary to maintain equilibrium far outweigh the disadvantages.

Note.—Professor Wiltshire informs me that many years ago, Mr. Mitchell, at the Royal Institution, showed a model by which the derivation of the crystalline systems from the octahedron was explained.

EXPLANATION OF PLATE II.

Fig. 1.— $\alpha', \beta', \gamma', \delta', \epsilon', \eta'$, octahedron.
 p, p' , weights.

$\alpha, \beta, \gamma, \delta, \epsilon, \zeta, \eta, \theta$, double hexagonal pyramid.
Fig. 2.— a, b, c, d, e, f , octahedron.
 $a', b', c', d', e', f', g', h'$, cube.

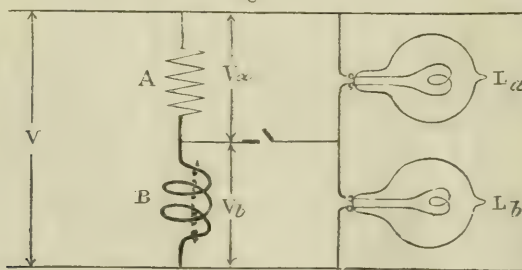
A, B, C, D, E, F , weights.
Fig. 3.—The upper figure shows the hexagonal prism surmounted by hexagonal pyramids. The lower shows the rings with the rhombohedron formed.

XIX. A Shunt-Transformer. By Mr. E. W. SMITH*.

ALTHOUGH this experiment has already been described by Professors Ayrtton and Perry in a paper at the Institution of Electrical Engineers, it was thought to be worth while occupying this Society's time in showing it here, as it forms a good lecture-experiment, if nothing more, to illustrate acceleration and lag of alternating currents.

The experiment consists as follows:—Between two leads a

Fig. 1.

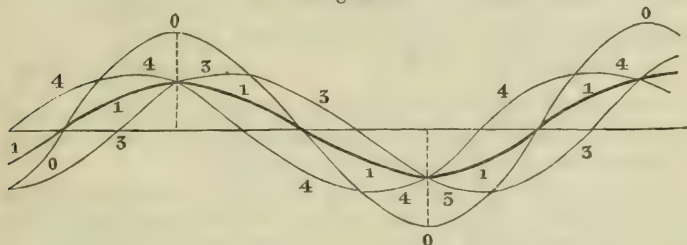


certain alternating potential difference, V , is maintained. We have two resistances, A and B (fig. 1), in series, through

* Communicated by the Physical Society: read June 8, 1889.

which part of the current flows. If the impedance of A is equal to that of B, then P.D. at terminals of A, V_a , and P.D. at terminals of B, V_b , are equal. If A and B are alike in resistance and self-induction, then V_a and V_b would be in the same phase and each equal to $\frac{1}{2} V$. If curve 0 0 0 0 (fig. 2)

Fig. 2.



represents V , then curve 1 1 1 1 represents $\frac{1}{2} V$. But if A has large resistance and B much self-induction, then, although V_a may be still equal to V_b , V_a will be lagged and V_b accelerated. Then curve 3 3 3 3 will represent V_a , and 4 4 4 4 V_b .

We have also two similar sets of lamps, L_a and L_b (fig. 1) in series between the two main leads. Under ordinary circumstances they would each have a P.D. of $\frac{1}{2} V$ (curve 1 1 1 1, fig. 2) at their terminals. But if we connect the junctions of the two sets of lamps with the junction of the two resistances, then lamp L_a will have P.D. V_a , and lamp L_b a P.D. V_b ; and therefore *both lamps will become brighter*. If the lamps take an appreciable current, then, when the junctions of the different circuits are joined, the current in the inductive resistance as well as in the lamps becomes greater, but that in the *main circuit becomes smaller*, as may be seen by a dynamometer.

The resistance A may of course consist of lamps, and B may be a choking coil, which absorbs very little energy.

Prof. Ayrton has given this inductive part of the circuit the name of a "Shunt-Transformer."

I have made a similar experiment with one of Mr. Mordey's transformers wound with three coils, each having the same number of turns. Using one of these as a primary and the other two as two independent secondaries, then, by having one circuit comparatively non-inductive and the other inductive, one gets the arithmetic sum of the amplitudes of the secondary currents *greater than that of the primary current*, although of course the vector sum must be less. For instance, take one particular experiment. The primary was on a circuit having a P.D. of 128 volts. One secondary, A, was composed of lamps, and the other, B, was a Tesla motor.

Volts in primary,	128	Current in primary,	14.2
„ secondary A,	119	„ secondary A,	8.0
„ secondary B,	119.5	„ secondary B,	8.2
		} 16.2	

Connecting up the secondaries of transformers A and B in parallel, the volts at the terminals of the primary being the same as before, and the secondary circuit being lamps.

Volts in primary,	128	Current in primary,	17.4
„ secondary,	121	„ secondary,	16.3

Here we have to give 17.4 amperes to the primary instead of 14.2 to get same current in secondary, and the volts in secondary are practically the same as before.

If we were being supplied with electricity, what should the meter measure? Surely the amount of energy we use. But ordinary meters only measure ampere-hours, and so cannot but give records in favour of consumer or supplier. The sooner the public understand this, the sooner we shall have a scientific meter in our houses.

XX. *On the Use of the Biquartz in determining the position of the Plane of Polarization.* By A. W. WARD*.

THE biquartz has been so often used, especially on the Continent, by investigators on the rotation of the plane of polarization of light, and with such extremely varying degrees of accuracy, that it seems of interest to account for these results mathematically. Verdet and H. Becquerel obtained results which varied by less than $4'$; while Wertheim, Matteucci, Edlund, Lüdtge, and Villari obtained results varying by as much as 2° . Lüdtge has in one case obtained a rotation of 4° where, on his own showing, the light was circularly polarized. Verdet's and Becquerel's accurate results were obtained when rotation was looked for in liquids and isotropic substances; and the inaccurate results of Wertheim &c. were obtained when seeking for a rotation in doubly-refracting substances. In the former case the light remained plane-polarized, in the latter it became elliptically-polarized, and the position of the plane of polarization was really that of one of the axes of the ellipse. In the present investigation we shall then determine with what degree of accuracy the biquartz can be used to determine the position of the axes in elliptically-polarized light.

Let us suppose that the elements of the elliptically-polarized

* Communicated by the Physical Society: read June 8, 1889.

light are given by the displacements along the axes of the ellipse, and by the inclination of an axis of the ellipse to some direction fixed in space. Let the displacements ξ and η be parallel to the axes of the ellipse, and let the axes of x and y be fixed in space, z being the axis along which the light travels; and let ω be the angle between the axes of x and ξ . If then c^2 be the intensity of the light, $\tan \gamma$ the ratio of the axes of the ellipse, the vibrations of the light are given by the equations

$$\left. \begin{aligned} \xi &= c \cos \gamma \cos \frac{2\pi}{\lambda} (vt - z), \\ \eta &= c \sin \gamma \sin \frac{2\pi}{\lambda} (vt - z). \end{aligned} \right\} \dots \dots (1)$$

The angles γ and ω are known whenever we know the history of the light; how it became converted from plane-polarized into elliptically-polarized. If, for instance, the change took place in passing through a doubly refractive medium whose axes are those of x and y , then

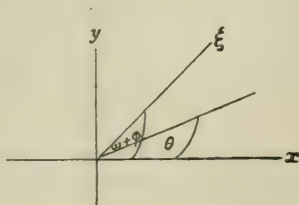
$$\left. \begin{aligned} \tan 2\omega &= \tan 2\alpha \cos \beta, \\ \sin 2\gamma &= \sin 2\alpha \sin \beta, \end{aligned} \right\} \dots \dots (2)$$

where β is the total angular retardation, and α the inclination of the initial plane of vibrations to that of xz . In these equations β is a function of λ , viz. $\frac{2\pi}{\lambda} (\mu_1 - \mu_2) z$, where μ_1 and μ_2

are the indices of refraction along the axes of x and y respectively. If α is small, variations in ω due to λ are not important; but if α is large this is no longer the case, as we shall eventually see.

Let us now pass the light (1) through a biquartz which is such that the plane of polarization of light, of wave-length λ , is turned through an angle ϕ . This rotation will simply turn the ellipse as a whole, and not affect the ratio of the axes. Hence for upper half of the biquartz ω becomes $\omega + \phi$, and for lower half $\omega - \phi$.

Let the light be now analysed by a Nicol whose plane of vibrations makes an angle θ with the plane of xz . If then k^2 be intensity of light passing through the upper half of biquartz, and k'^2 that of light passing through the lower half, we have, as usual,



$$\left. \begin{aligned} h^2 &= c^2 \cos^2 \gamma \cos^2 \overline{\omega + \phi - \theta} + c^2 \sin^2 \gamma \sin^2 \overline{\omega + \phi - \theta}, \\ k^2 &= c^2 \cos^2 \gamma \cos^2 \overline{\omega - \phi - \theta} + c^2 \sin^2 \gamma \sin^2 \overline{\omega - \phi - \theta}; \end{aligned} \right\} \quad (3)$$

or

$$\left. \begin{aligned} h^2 &= \frac{c^2}{2} \{1 + \cos 2\gamma \cos 2(\omega + \phi - \theta)\}, \\ k^2 &= \frac{c^2}{2} \{1 + \cos 2\gamma \cos 2(\omega - \phi - \theta)\}. \end{aligned} \right\} \quad \dots \dots \dots (4)$$

We have now to determine what value of θ makes $h^2 = k^2$ for all values of λ .

Equating h^2 to k^2 , we get

$$\cos 2\gamma \sin 2\phi \sin 2\overline{\omega - \theta} = 0. \quad \dots \dots \dots (5)$$

This equation is satisfied whenever

$$\cos 2\gamma = 0, \quad \dots \dots \dots (6)$$

or

$$\sin 2\phi = 0, \quad \dots \dots \dots (7)$$

or

$$\sin 2(\omega - \theta) = 0. \quad \dots \dots \dots (8)$$

The first of these solutions occurs when $\gamma = \frac{\pi}{4}$, *i. e.* when the elliptically-polarized light is really circularly polarized. In this case the phrase plane of polarization has no meaning at all, and so it need not be discussed.

The second solution (7) gives $\phi = \frac{\pi}{2}$. This can only be the case for one particular wave-length, and depends simply on the thickness of the biquartz. A biquartz is usually made of such a thickness that ϕ is $\frac{\pi}{2}$ for the yellow light from the brightest part of the spectrum. We shall suppose this to be the case here.

The third solution gives

$$\omega = \theta.$$

If, then, this solution does not hold for all values of λ , then, however the analysing Nicol be turned, both halves of the biquartz can never be made of the same uniform tint.

Now, considering the equation

$$\tan 2\omega = \tan 2\alpha \cos \beta,$$

we see that $\omega = \alpha$ always if $\beta = 0$, that is, if the incident plane-polarized light always remains so. If β is not equal to 0, then still $\omega = \alpha$ for all values of λ , if $\bar{\alpha} = 0$ or $\frac{\pi}{4}$. If α is 0, then

$\gamma=0$, and the light is plane-polarized as before. The case we have to discuss, then, is $\alpha=\frac{\pi}{4}$.

If $\alpha=\frac{\pi}{4}$, and also for any particular value of λ , $\beta=\frac{\pi}{4}$, then light of that colour is circularly-polarized. Hence, however we alter θ , no change in the intensity of that light will take place. If this circularly-polarized light comes from a prominent part of the spectrum, it will be impossible to note small change in the tint of passage due to the varying presence of other colours. The difficulty experienced will be precisely similar to that of fixing the position of the plane of polarization by means of the yellow tint of passage instead of the violet tint.

If β is never so great as $\frac{\pi}{2}$, then, when both halves of the biquartz are of the same uniform tint, the position of the analyser determines the position of the ellipse; but the uniform tint will not be that due to excluding the yellow light of the spectrum, but will contain lights of every colour, but not in that proportion which constitutes white light. The tint may be rosy or yellow.

If α is neither 0 nor $\frac{\pi}{4}$, then ω varies with λ , and cannot possibly satisfy the solution for all values of θ . In this case, then, both halves of the biquartz can never be made of the same tint. As this is the general case, we conclude that the biquartz is not a suitable instrument to use when, instead of plane-polarized light, we have elliptically-polarized.

The following table gives the values of ω due to variations in α and λ when the light has passed through a quarter undulation-plate of quartz. The values have been calculated from Rudberg's table of indices, quoted on p. 317 of Glazebrook's 'Optics.' The capital letters refer to the lines of the spectrum.

		C.	D.	E.	G.
	β .	80°.	90°.	100°.	126°.
$\alpha=10^\circ$.	ω	2°	0	-2°	-6°
$\alpha=20^\circ$.	ω	4°	0	-4°	-13°
$\alpha=44^\circ$.	ω	39°	0	-39°	-43°

The above is simply given as an illustration of the magnitude of the quantities involved in a particular case where it is easy to make the calculations. I have tried the experiment

by passing light the reverse way through an elliptic analyser (*i. e.* a Nicol prism and quarter undulation-plate), then through the biquartz, and finally through an analyser. It is found quite impossible to get any match between the two halves of the biquartz when α is large. The actual dispersion of the axes depends upon the variations of β with λ , and this is very much greater in quartz than in such a doubly refracting substance as compressed glass. But in most cases there will always be sufficient variation to make the use of the biquartz a very unsuitable method, and this does, I think, account for the two classes of results mentioned at the beginning of this paper.

In conclusion, I have only to express my gratitude to Mr. Glazebrook for many valuable suggestions, and to Professor Thomson for the use of the Cavendish Laboratory.

XXI. Notices respecting New Books.

A Treatise on Spherical Trigonometry, and its application to Geodesy and Astronomy; with numerous examples. By Dr. J. CASEY, F.R.S. (Longmans: 1889.)

DR. CASEY has "struck oil" as a writer of Mathematical Text-books. It is not so many years since he began this career with his useful and excellent 'Sequel to Euclid,' which has now reached a fifth edition, and since that time he has produced other text-books of like good quality. Having given us an "elementary" and a more advanced Plane Trigonometry, he now completes this special corner of mathematical literature with the work before us. The student will find here all, or nearly all, he wants in a text-book on the subject, illustrated by much matter selected from foreign periodicals, with variety of proofs. Following a practice which has come much into vogue of late years, many results are ticketed with the names of the earliest publishers of them: for instance, two formulæ which frequently occur in the solution of triangles are called the first and second Staudtans of a triangle. Recent points and lines which occur in Plane Geometry, and which have analogues in Spherics, have the like names here: some, as the Lemoine point and the Symmedian point, which are identical in plane, do not coincide in Solids. The specially noteworthy chapters, as might be expected from Dr. Casey's original work in this field, are, in our opinion, those upon the small Circles on the Sphere and on Inversions. There is a large collection of exercises, and, after the author's previous manner, the more noteworthy results are numbered; of these, 495 are given. In addition to a handy and compact account of the purely Trigonometrical details, there is a final chapter on the applications to Geodesy and Astronomy. The text is accompanied by a short index.

XXII. *Proceedings of Learned Societies.*

GEOLOGICAL SOCIETY.

[Continued from p. 71.]

May 22, 1889.—W. T. Blanford, LL.D., F.R.S., President,
in the Chair.

THE following communications were read:—

1. "Notes on the Hornblende Schists and Banded Crystalline Rocks of the Lizard." By Major-Gen. C. A. McMahon, F.G.S.

The Lizard district has been visited by the author on three occasions during the years 1887–8–9, and the specimens of the rocks collected were subjected to microscopic examination. After summarizing the work of previous writers, the author proceeded to consider the hornblende schists. He described these rocks and gave a table showing their constituent minerals. He noted the absence of quartz, the presence of pyroxene, and the fact that the minerals present are those commonly met with in volcanic rocks either as original minerals or as secondary products, and he considers that the microscopic study of the schists confirms the opinion of some previous writers that the schists had a volcanic origin and consisted principally of ash-beds. The absence of free quartz militates strongly against the supposition that they were originally sedimentary rocks of an ordinary character, whilst the fact of their being bedded shows that they are not plutonic. The author has found no evidence that the foliation of these rocks is due to dynamic deformation, and gives reasons for supposing that such was not the case. The rock seems to have been originally homogeneous, and its banding produced at a later stage by the segregation of the hornblende in planes parallel to the bedding.

The rocks furnish abundant evidence of the action of water, as shown by the presence of calcite, chlorite, steatite, and other products of aqueous action, as well as by channels fringed with magnetite, ferrite, or limonite. The action of water in converting augite into hornblende may be distinctly traced when the slices still contain pyroxene. The production of periodical currents of water through the water-bearing strata adjoining the roots of a volcano was commented on, and the author suggested that the banding of the hornblende schists was produced by such water leeching out unstable minerals, such as pyroxene, from the spaces between the planes of lamination, and the formation of comparatively stable minerals, such as hornblende, along those planes. The Lizard rocks contain good examples of the formation of hornblende in the wet way, that mineral having been deposited in cracks in such a way as to join together the ends of hornblende crystals severed by these cracks.

The "granulitic" group, of which the author gave a table showing the constituent minerals, was then described. Judged by

their mineralogical contents the dark bands consist of diorite and the white bands of granite.

The author considers that portions of this group consist, like the hornblende schists, of converted ash-beds, but that other portions are composed of intrusive diorites of later date, the quasi-bedded appearance of both being due to the injection of granite. He pointed out that the quasi-banding is very irregular in its character, that the bands inosculate, bifurcate, and entangle themselves in complicated meshes inconsistent with the idea of regular banding, and that they are deflected by the blocks of serpentine imbedded in the dioritic portions of the granulitic rocks as well as by the porphyritic crystals of felspar contained in the latter. In certain places, as on the foreshore at Kennack Cove, the intrusive character of the granitic veins is undoubted, as they cut through the diorite in all directions, but they graduate into bands of normal character. The author considers that the process of injection was aided by the plasticity of the "granulitic" beds induced by the neighbourhood of igneous masses; also in the case of submarine ash-beds by the planes of sedimentation, and in the case of intruded sheets of diorite by the foliation parallel to the bedding, the intrusion of the granite being subsequent to that of the diorite.

At Pen Voose a foliated granite, the author pointed out, occurs in association with a non-foliated gabbro and diorite, a fact indicating in his opinion that the foliation of the granite was produced before its perfect consolidation. The granite was the last to appear in the order of time, and had the foliation of the granite been produced by pressure after cooling, the gabbro and diorite would also have been foliated.

2. "The Upper Jurassic Clays of Lincolnshire." By Thomas Roberts, Esq., M.A., F.G.S.

In Lincolnshire it has generally been considered that the Oxford and Kimeridge Clays come in direct sequence, and that the Corallian group of rocks is not represented. The author, however, endeavoured to show that there is between the Oxford and Kimeridge a zone of clay which is of Corallian age.

Six palæontological zones were recognized in the Oxford Clay. The clays which come between the Oxford and Upper Kimeridge the author divided into the following zones:—

- (1) Black seleniferous clays.
- (2) Dark clays crowded with *Ostrea deltoidea*.
- (3) Clays with *Ammonites alternans*; and (4) clays in which this fossil is absent.

The black seleniferous Clays (1) are regarded as Corallian, because

- (a) They come between the Oxford Clay and the basement bed of the Kimeridge.
- (b) Out of the 23 species of fossils collected from this zone 22 are Corallian.

- (c) *Ostrea deltoidea* and *Gryphæa dilatata* occur together in these clays, and also in the Corallian, but in no other formation.

The zones 2, 3, and 4 are of Lower Kimeridge Clay age. The lowest zone (2) is very persistent in character, and is met with in Yorkshire, Cambridgeshire, Oxfordshire, and the south of England. The remaining zones (3 and 4) are local in their development.

3. "Origin of Movements in the Earth's Crust." By James R. Kilroe, Esq.

The author is convinced that a very important factor has been omitted from the usual explanation offered in accounting for the vast movements which have obtained in the Earth's crust. His acknowledgments are due to Mr. Fisher for the extensive use made of his valuable work. He also refers frequently to the views and publications of other writers on terrestrial physics. From a somewhat conflicting mass of figures he concludes that about 20 miles would remain to represent the amount of radial contraction due to cooling during the period from Archæan to Recent times, corresponding to a circumferential contraction of 120 miles. This will have to be distributed over widely separate periods, at each of which there is abundant evidence of lateral compression.

But he considers that this shrinkage alone will not account for all the plication or distortion of strata which constitute so important a factor in mountain-making, and he is disposed to supplement it in the way to which allusion has already been made by Mr. Wynne in a recent Presidential Address, viz. by considering the effects of the attenuation of strata under superincumbent pressure from deposition in subsiding areas, which involves the thickening, puckering, reduplication, and piling up of strata in regions where pressure has been lessened. It should be noted that, until disturbance of "cosmical equilibrium" takes place, mere pressure does not produce metamorphism. The extent of these lateral movements is described, and it is asserted that the theories hitherto adopted to account for plication, &c. are inadequate.

The origin of the horizontal movements is further discussed on the hypothesis that solids can flow after the manner of liquids, when they are subjected to sufficient pressure. He considers that the displacement in N. W. Scotland may have been initiated by the force due to contraction and accumulating in the crust throughout the periods marked by the deposition of Torridon Sandstone and Silurian strata, the elements of movement finding an exit at the ancient Silurian surface. In this case the pile of Silurian strata formerly covering the region now occupied by the North Sea and part of the Atlantic forced the lowest strata to move laterally, the protuberances of the underlying pre-Silurian rocks being also involved in the shearing process. Similar results obtain in other mountain areas. The strata compressed have been greatly attenuated, and extended in proportion; in this way we may account for the piling up of strata by contortion in certain regions. The connexion

of this interpretation with Malet's theory of volcanoes is also indicated, and the author concludes by applying these views to other branches of terrestrial physics.

June 5.—Prof. J. W. Judd, F.R.S., Vice-President,
in the Chair.

The following communications were read:—

1. "Observations on some undescribed Lacustrine deposits at Saint Cross Southelmham, in Suffolk." By Charles Candler, Esq.

2. "On certain Chelonian Remains from the Wealden and Purbeck." By R. Lydekker, Esq., B.A., F.G.S.

3. "On the Relation of the Westleton Beds or Pebbly Sands of Suffolk to those of Norfolk, and on their Extension inland." By Prof. Joseph Prestwich, M.A., D.C.L., F.R.S., F.G.S.

Part I.

The author in this, the first part of his paper, described the Westleton beds of the East Anglian coast. He commenced with a review of the work of previous writers, especially Messrs. Wood and Harmer, and the members of H.M. Geological Survey, including Messrs. H. B. Woodward, Whitaker, and Clement Reid. In discussing this work, particular attention was paid to the Bure-valley beds, which were considered as a local fossiliferous condition of the Pebbly Sands; but the term is not so applicable to these sands as that of the "Westleton and Mundesley Beds," which the author proposed in 1881.

The Westleton beds were carefully described, as seen in coast-sections in East Anglia, proceeding from south to north, and the following classification was adopted:—

- | | | |
|---|---|--|
| <p><i>The Westleton
and Mundesley
series</i>
(The Mundesley
section of it).</p> | } | <p>1. Laminated clays, sand, and shingle with plant-remains and freshwater shells (the Arctic forest-bed of Reid).</p> <p>2. Sand and quartzose shingle with marine shells (the <i>Leda myalis</i> bed of King and Reid).</p> <p>3. Carbonaceous clay and sands with flint-gravel and pebbles of clay, driftwood, land and lacustrine shells and seeds (the Upper freshwater bed of Reid).</p> |
| <p><i>The Forest-bed
series of Reid</i>
(exclusive of No. 3
of above).</p> | } | <p>4. A greenish clay, sandy and laminated in places, containing abundant mammalian remains, and driftwood, with stumps of trees standing on its surface (the forest- and elephant-bed of authors; the estuarine division, in part, of Reid).</p> <p>5. Ferruginous clay, peat, and freshwater remains and gravel (the Lower freshwater bed of Reid).</p> |

The Westleton beds were found to rest with discordance on various underlying beds; in places on the Forest series, elsewhere on the Chillesford Clay, whilst occasionally the latter had been

partly or entirely eroded before the deposition of the Westleton beds. In the north, where the present series dies out, they come in contact with the so-called Weybourn Crag, which the author supposed to be the equivalent of the Norwich Crag. A similar discordance has been noted between the Westleton beds and the overlying glacial beds, so that the former mark a distinct period, characterized by a definite fauna, and by particular physical conditions. The Westleton beds being marine, and the Mundesley beds estuarine and freshwater, the author proposed to use the double term to indicate the two facies, as has been done in the case of other deposits. But these facies were found to be local, and the most persistent feature of the beds is the presence of a shingle of precisely the same character over a very wide area. By means of this the Westleton beds can be identified far beyond East Anglia, and where there is no fossil evidence, and they throw considerable light on important physiographical changes.

The author described the composition of the shingle, which, unlike the glacial deposits, contained pebbles of southern origin.

The paper concluded with a list of fossils, excluding those of the Forest-bed (the stumps of which, the author considered, were frequently in the position of growth). Should the Forest-bed eventually prove to be newer than the Chillesford beds, it was maintained that the former must be included in the Westleton series, and its flora and fauna added to the list, whilst if, on the contrary, the Forest-bed should be proved synchronous with the Chillesford beds it must be relegated to the Crag.

The second part of this paper will treat of the extension of these beds into and beyond the Thames Valley, and on some points connected with the physical history of the Weald.

June 19.—Prof. J. W. Judd, F.R.S., Vice-President,
in the Chair.

The following communications were read:—

1. "On Tachylyte from Victoria Park, Whiteinch, near Glasgow."
By Frank Rutley, Esq., F.G.S.

This paper dealt with the microscopic characters of certain thin tachylytic selvages occurring on the margins of white-whin (basalt) veins which traverse Carboniferous shales in Victoria Park, and which have already been described in some detail by Messrs. John Young and D. Corse Glen. The white-whin veins, which sometimes are not more than an inch in breadth, are found to become gradually more vitreous in passing from the middle to the sides of the veins. Near the margin they become densely spherulitic, the spherulitic band on either side of the vein being followed by a less spherulitic and more glassy band, the vitreous matter of which appears nearly or quite colourless. A sharp but irregular boundary-line follows, beyond which lies a band of a more or less deep brown or coffee-coloured glass which the author considers to have resulted from the

fusion of the shale, two narrow vitreous bands of different origin being thus developed side by side on each side of the vein, the colourless bands representing the chilled margins of the vein, the brown bands the fused surfaces of the walls of shale. The author only suggested this as a plausible explanation of the microscopic phenomena. An analysis of portion of one of these whin veins with its adherent tachylite, made by Mr. Philip Holland, was appended to the paper.

2. "The Descent of *Sonninia* and of *Hammatoceras*." By S. S. Buckman, Esq., F.G.S.

3. "Notes on the Bagshot Beds and their Stratigraphy." By H. G. Lyons, Esq., R.E., F.G.S.

The author deplored the necessity of quitting the area which he had studied before completing his observations, and wished to place his results at the disposal of other workers.

In a previous paper he had discussed the beds at their southern outcrop, over a small area, and showed that there the Bagshot and London Clay strata remained of constant thickness, and dipped northwards at an angle of about $2\frac{1}{4}^{\circ}$. He had since examined the country between Aldershot and Ascot over an area of about 15 miles square, and attempted by contouring the surface of the Middle Bagshot beds (which showed a nearly constant thickness of 60 feet over the area), to give the form into which the beds had been pushed by the different slight flexures which might occur. After giving details of the heights at which this surface was found, he concluded that an anticlinal of which the axis pointed upon Windsor Castle, appeared to pass through the Swinley and Welington-College area, and probably to Hazeley Heath; and that a synclinal started by Minley and Hawley, and ran by the Royal Albert Asylum, Gordon Boys' Home, upon Ongar and Row Hills, and Woburn Hills; and that another anticline ran to St. George's Hill, Weybridge.

The author had attempted to map the southern and eastern limits of the Upper Bagshot beds, and claimed a much greater extent for these beds in those directions than had been assigned by the members of the Geological Survey. The outcrop of the beds was described in some detail, and the occurrence of outliers on Knaphill Common, by Donkey Town, on Chobham Common, and on Staples Hill was noted.

4. "Description of some new Species of Carboniferous Gastropoda." By Miss Jane Donald.

5. "*Cystechinus crassus*, a new Species from the Radiolarian Marls of Barbadoes; and the evidence it affords as to the Age and Origin of those Deposits." By J. W. Gregory, Esq., F.G.S.

XXIII. *Intelligence and Miscellaneous Articles.*

ON THE KINETICS OF BODIES IN SOLUTION. BY W. NERNST.

SINCE Van t'Hoff disclosed the great analogy of the constitution of dissolved bodies in dilute solution with the gaseous state, it becomes possible, as the author shows, to explain diffusion on purely mechanical principles. The most essential difference from gaseous diffusion lies in the much smaller velocity of diffusion of solutions, which justifies the inference that the solvent offers an enormous resistance to the moving molecules.

The author investigates in the first place the diffusion of non-electrolytes. Here the driving force is solely the alteration of osmotic pressure p with the locality x . Since p is proportional to the concentration c (number of g -molecules in 1 cub. centim.), and thus $p = p_0 c$, we get for the quantity of substance in g molecules, which travels through the section q of a cylinder in the time z :—

$$S = - \frac{qz p_0}{K} \frac{dc}{dx}.$$

K is the force which imparts unit velocity to a molecule in solution. This law, which is of the same form as the well-known one stated by Fick, renders it possible to calculate K in absolute measure, as the author shows by a few examples.

Still more interesting is the calculation of the diffusion for solutions of electrolytes; for the coefficient of diffusion may here be calculated in absolute measure, on the basis of the hypothesis of dissociation propounded by Arrhenius and others. By means of Ohm's law, Kohlrausch, as is well known, has calculated from the galvanic migration of the ions, which is solely due to electrostatic forces, the force which imparts to a g -ion in aqueous solution unit velocity of migration. If now, in the diffusion of an electrolyte the inequality of osmotic pressure were the only driving force, then, from the different mobility of the positive and the negative ions (*e. g.* H and Cl), free electricity would at once form in the solution. This is prevented by the establishment of an electrostatic force, the action of which has just the result that the ions in the solution are present in equivalent ratios. From this condition the magnitude of that force may be calculated, and retaining the same nomenclature as above, we get for the actual quantity of the anion or kation diffusing in unit time:—

$$S = -1.121 \cdot 10^{-4} q z p_0 \frac{2uv}{u+v} \cdot \frac{dc}{dx};$$

u and v are the molecular conductivities in mercury units. From this formula we have for the centimetre, the day, and 18° :—

$$K = \frac{uv}{u+v} 0.04768 \cdot 10^7.$$

For the sake of comparison with experiment, the influence of temperature on the coefficient of diffusion is taken into account on the basis of the theory of dissociation: the coefficients 0.026 for salts and 0.024 for acids and bases represent the alteration of conductivity with temperature; these numbers are found to agree very well with the observations of de Heen and Schumeister.

The coefficients of diffusion reduced to 18° agree very well with the observations of various experimentalists, as shown in the table given; and this agreement is an excellent proof of the validity of the author's consideration.

From the same point of view the author treats the diffusion of a mixture of salts, as well as of electrolytes at greater concentration. He shows further, how in the same way the difference of electrical potential between solutions of different concentration may be calculated. Between two places of a solution in which the osmotic pressure is p_1 and p_2 , the difference of potential is $p \frac{u-v}{u+v} \log \frac{p_1}{p_2}$.

Zeitschrift für phys. Chemie [2] vol. ii. p. 613; *Beiblätter der Physik*, vol. xiii. p. 131.

ON THE MOLECULAR CONDITION OF DISSOLVED IODINE.

BY MORRIS LOEB.

By means of a determination of the vapour tensions, the author endeavoured to determine whether iodine in its brown solution is in a different molecular condition to that of its violet solution. Ether was used as a solvent for the brown, and bisulphide of carbon for the violet. The measurement of the tension was effected by means of a Regnault's apparatus, suitably modified. A trial of its applicability, by means of solutions of naphthaline &c. in ether and in bisulphide of carbon, gave satisfactory results, since the molecular weights 132 and 127.5 were obtained instead of 128. The mode of calculation is given in the original. For solutions of iodine in bisulphide of carbon, the median values of the separate series of experiments varied between 264 and 326.6, the general mean was found to be 303.25. For molecules of the composition I_3 the molecular weight is calculated at 254, and for I_2 at 381; the number obtained is about the mean of these.

The solution of iodine in ether also gave very divergent values for the individual series of experiments, as they varied between 466.1 and 577.2, while the general average 507.2 agrees almost entirely with the value 508 required for I_4 . The existence of the molecules I_4 appears therefore probable for the brown modification.

Experiments made to determine the question by means of Raoult's freezing-point method were unsatisfactory, for within the limits caused by the difficult solubility of iodine the errors of

observation are too great.—*Zeitschrift für phys. Chemie*, vol. ii. p. 206; *Beiblätter der Physik*, vol. xiii. p. 134.

SOME OBSERVATIONS ON THE PASSAGE OF ELECTRICITY
THROUGH GASES AND VAPOURS. BY DR. NATTERER.

The experiments were made with the aid of an induction apparatus, and special regard was had to the sparking distance of the electrical discharges, to their luminosity, and to the extension of the glow-light which occurs at the negative electrode under diminished pressure. It appears that these three phenomena, which are characteristic for each individual gaseous body, are in relation with the number of atoms in the molecule, and with the molecular weight.—*Sitzungsberichte der Wiener Akademie*, June 21, 1880.

ON THE ELECTRICAL RESISTANCE OF INSULATORS AT HIGH
TEMPERATURE. BY DR. H. KOLLER.

This is a continuation of a paper by the same author on the passage of electricity through very bad conductors. It forms two parts.

In the first, the author investigates the connexion between the electrical conductivity of some liquid insulators, and their fluidity at various temperatures; it was found that the course of these two properties is parallel, but not proportional. The conductivity always increases more rapidly than the fluidity, so that, for instance, with petroleum ether a twelvefold increase of the conductivity corresponds to only a threefold increase of the fluidity. The conductivity of those substances exhibits the greatest increase when their fluidity also increases most strongly with the temperature. Castor-oil, for instance, conducts 350 times better at 132° than at 20°, while between these two limits its fluidity increases by only 43 times.

The second part deals with the gradual change which an imperfect dielectric experiences in consequence of rise of temperature. The author concludes, partly from Hopkinson's and partly from his own experiments, that the first effect of the increase of an imperfect dielectric consists in the fact that it begins to form residues. The formation of the residue is at first of very short duration, but with increase of temperature extends over a longer interval of time, and the residues developed adhere in accordance with this continually more firmly to the dielectric. At still higher temperatures they are rapidly altered into a form in which a reverse change with free electricity is only possible with difficulty and with great loss; and is finally not possible at all.

The experimental result is that in a condenser formed of the dielectric in question the amount of residue which can be demonstrated—that is, that which neither takes part in a discharge of

short duration, nor is so similar to heat that it cannot change into dielectric displacement—increases on heating from approximately zero to a maximum. It decreases after this, and with the occurrence of perfect conduction it entirely disappears.—*Sitzungsberichte der Wiener Akademie*, June 21, 1889.

ON THE RESISTANCE TO DISRUPTIVE DISCHARGE OFFERED BY
GASES UNDER HIGH PRESSURES. BY MAX WOLF.

At the instance of Prof. Quincke the author attempted to ascertain what resistance certain gases offered at high pressure to the passage of the electrical spark. In other words, the difference of potential of two spherical surfaces was determined at the moment of the discharge, for different gases and at various pressures greater than one atmosphere. In this a method was used similar to that used by Quincke for determining striking distances in insulating liquids. It was to be expected that under higher densities the irregularities in the discharge occurring under smaller pressures must be less prominent.

The conclusions arrived at are as follows :—

(1) The electrical force which produces the disruptive discharge in various gases between spherical surfaces of 5 centim. radius and at a distance of 0.1 centim. increases proportionally to the pressure for pressures between 1 and 9 atmospheres.

(2) The increase of the electrical force for simpler gases (oxygen, hydrogen, and air) is inversely proportional to the mean path of the gas-molecules.

(3) With carbonic acid the product from the increase in the electrical force into the mean path for an increase of pressure for one atmosphere is considerably smaller (almost one half) that of simple gases.

(4) One or more discharges are necessary until the resistance of a gas is attained, and the resistance is at first so much the less than in the later discharges, the higher is the pressure on the gas.—Wiedemann's *Annalen*, vol. xxxvii. p. 306 (1889).

THE NATURE OF SOLUTIONS. BY S. U. PICKERING.

On pages 36–38 of this Magazine Prof. Arrhenius publishes a criticism of my paper on this subject. I venture to think that it is somewhat rash of Prof. Arrhenius to attack a paper which has not yet been published, and of which only a short abstract, destitute of all experimental data, has as yet appeared in print. If he will wait till the paper be published in full (and it may be some months yet before it is so) he will, I think, find that several of his criticisms are mistaken, and that the others have already been answered. If otherwise, I shall then be ready to answer him on any point which he may raise.

July 2, 1889.

THE
LONDON, EDINBURGH, AND DUBLIN
PHILOSOPHICAL MAGAZINE
AND
JOURNAL OF SCIENCE.

[FIFTH SERIES.]

SEPTEMBER 1889.

XXIV. *On the Mechanics of Luminosity.*
By E. WIEDEMANN*.

[Plate III.]

ALTHOUGH we possess numerous measurements of the positions of lines in the spectrum, and although many attempts have been made to measure the distribution of luminosity and energy in the spectrum, and to express the same by means of formulæ, yet the experiments are few which have for their object to obtain an insight into the mechanics of luminosity. In general, we have contented ourselves with investigating the intensity of radiation without investigating the energy of the vibrating particles which emit the light, upon which it depends.

In what follows we shall attempt to fix the data for investigating the mechanics of luminosity, to verify particular conclusions by means of experiment, and to determine the numerical value of the quantities which occur.

The present research is a continuation of earlier investigations of mine†.

* Wied. *Ann.* xxxvii. p. 177. Translated from a separate impression communicated by the Author.

† E. Wiedemann, Wied. *Ann.* v. p. 506 (1878); vi. p. 298 (1879); ix. p. 157 (1880); x. p. 202 (1880); xviii. p. 508 (1883); xx. p. 756 (1883); xxxiv. pp. 446 and 464 (1888). *Sitzungsber. d. Societas physio-medica Erlangen vom 1 Aug. 1887.* *Bull. Soc. de phys. généroise* 6 Oct. 1887. I shall return more at length to numerous obligations to the above-mentioned works—where no special acknowledgment is made—upon a future opportunity. The results of this investigation themselves were communicated to the Physico-Medical Society of Erlangen on Dec. 10, 1888.

Phil. Mag. S. 5. Vol. 28. No. 172. Sept. 1889.

N

General.—Production of Light.

1. According to the newer views of the constitution of bodies, we assume motions of translation of the molecules with their centre of gravity ; further, rotation and oscillation round the centres of gravity considered as fixed, not only of the material parts of the molecules but also of their æther-envelopes. With *gases* the motions of translation of the centres of gravity produce only feeble emission of light. Sir G. G. Stokes* has sought in this the explanation of the feeble continuous spectrum of sodium seen at the same time as the line-spectrum. I have myself† endeavoured to show that the motions of rotation cannot be the cause of the line- and band-spectra. Consequently we must seek the cause of the production of light in gases in the intermolecular movements which occur within the molecule—either of their material particles or of their æther-envelopes. We shall endeavour to show later on that it can only be the vibrations of the material particles to which the emission of light is due. With *solid* and *liquid bodies* the emission of light may be due to the vibrations of the whole molecule about its position of equilibrium as well as to the intermolecular motions of the constituent atoms of the molecule. Upon the first depends the uniform continuous spectrum of all solid bodies when heated, and upon the last the differences between the light emitted by different bodies.

2. The following theoretical considerations rest upon the conception of *luminosity* introduced by me, and the discrimination of totally different phenomena which have generally been confused.

I designate as *light* the whole complex of rays between the infra-red and the extreme ultra-violet. The motions of the molecules which produce the luminosity I will call, for the sake of brevity, *luminous motions*, in contradistinction to the vibrations of the emitted waves of light.

I shall always designate as *intensity* of the light-vibrations the energy measured in gramme-calories per second which the vibrations of the light-waves emitted by the molecules of the body carry with them ; but as *luminous energy* the energy of those motions of the molecules or their atoms which produce the radiated light. The first energy depends upon the decrease of the second with the time. A chief problem of

* See a Research of A. Schuster, Phil. Trans. Lond. 1879, p. 37.

† Wied. *Ann.* v. p. 507 (1878).

this research is to show how the luminous energy may be determined from the intensity.

I shall speak of *brightness* when the intensity is measured by photometric methods, that is by a physiological method.

3. I take as the basis of my investigations the kinetic theory of gases. According to this there exists at constant temperature a perfectly definite relationship between the kinetic energy of the motion of translation, corresponding to the temperature, and that of the intermolecular motions, both as a whole as well as for each kind thereof, and consequently also between those of the motion of translation and the luminous motion; otherwise no permanent condition would be possible. This relationship may be regarded as the normal one. If, for any reason, the normal relationship is disturbed in any way, it will in time again become normal.

If, for example, we exalt the luminous motion in a molecule in comparison with the motion of translation, the first will be transformed into the latter; if the luminous motion becomes lowered by radiation the loss will be partly replaced from the store of energy of the motion of translation by means of the impacts of the molecules.

Luminescence and Temperature of Luminescence.

4. In special cases, however, the normal relationship between the motion of translation corresponding to the temperature and the luminous motion does not exist.

In a former paper I have ventured to employ the term *luminescence* for all those phenomena of light which are more intense than corresponds to the actual temperature.

In all phenomena in which luminescence is manifested the energy of the luminous motion is higher than that characterized above as corresponding to the relationships determined by the temperature alone. According to the mode of excitation I distinguish Photo-, Electro-, Chemi-, and Tribo-luminescence. In particular, *photo-luminescence*, including fluorescence and a number of cases of phosphorescence, is defined as those phenomena in which the incident light excites vibrations within the molecule of a body which produce directly an emission of light. I do not include in this those cases in which the incident light produces primarily chemical processes, upon which a production of light is secondarily dependent. This occurs for example with a large number of phosphorescent substances, *e. g.* calcium sulphide. These

phenomena apparently belong to photo-luminescence, but really to chemi-luminescence.

The *temperature of luminescence* is defined to be the temperature at which a body, heated without decomposition, would give light of a particular wave-length in each case of exactly the same brightness as it does in consequence of the processes of luminescence.

The production of light in the phenomena of luminescence in gases cannot be explained by assuming that in consequence of the different velocities of the individual molecules, as assumed by the kinetic theory of gases, the individual molecules possess a very high temperature, and therefore become luminous. For at the temperature of ignition, defined by the great velocity of the motion of translation, most substances would be decomposed. Certainly all organic substances, fluorescent or phosphorescent, in the gaseous condition. The like holds good for solid bodies and for liquids, only that here the limits within which the velocities of the molecules are included are much narrower than with gases.

The Temperature of Luminescence and the Second Law of the Thermodynamic Theory.

5. In a number of phenomena we have to take account of the temperature of luminescence.

In all mathematical developments which are based upon the second law of the thermodynamic theory expressions such as $\frac{dQ}{T}$ occur, involving quantities Q of heat communicated to or withdrawn from the body, divided by the absolute temperature T at which this takes place.

If luminescence occurs in consequence of this addition of energy, then the temperature corresponding to certain intermolecular motions, to be defined later on (39), upon which the luminescence depends, *i. e.* the temperature of luminescence, is much higher than the temperature of the luminescent body as measured by the thermometer*.

We must therefore divide the changes of energy which

* The assumption upon which deductions from the second law rest, viz. that heat cannot be conveyed from a body of lower temperature to one of higher temperature without the expenditure of work, must therefore be otherwise conceived in accordance with the above considerations upon the temperature of luminescence, since when phenomena of luminescence occur such a transference may very well take place, as I shall show more fully further on.

occur into two parts—the first, which corresponds to the prevalent mean temperature as defined by the motion of translation of the molecules; and the second, which is determined by the intermolecular motion. Consequently the expression $\int dQ/T$ must be divided into two parts corresponding to these two processes. If the luminescent light is not homogeneous, but if it consists of separate bright lines, or if it yields a continuous spectrum, then for each ray of light of definite wave-length emitted the temperature of luminescence and the corresponding quantity of energy must be determined. Each member of $\int dQ/T$ takes then the form $\sum dQ_x/T_x$ where T_x represents the temperatures defined by the motions of translation or by the internal motions, and dQ_x represents the corresponding quantities of heat. We must further remark that in the integral all the members whose temperature of luminescence is very high become very small, whilst according to the usual mode of treatment they have a considerable value.

6. That such phenomena of luminescence actually occur may be directly shown in numerous cases, as in gases which are made luminous by electric discharges without any corresponding elevation of temperature; further, in cases of *chemi-luminescence*, and indeed in processes where one would not have expected it. The experiments of W. von Siemens* have shown that gases heated far above 1000°C . emit no light; and yet an alcohol flame is luminous. It produces preeminently ultraviolet rays; in the combination of the constituents of the alcohol with the oxygen internal motions arise, for which the corresponding temperature of luminescence is situated far above the temperature of the flame. The case is similar with burning carbon disulphide, and sulphur, and in many other cases; so also with the luminosity of phosphorus at low temperatures, the emission of light by arsenious acid upon crystallization attended with rearrangement of its molecules, &c.

7. In these phenomena of luminescence the occurrence of internal motions of other temperature than that shown by the thermometer may be perceived immediately by the eye. Hence the necessity of dividing the quantities of heat into two parts is at once apparent. But analogous processes occur in many other cases, as in most chemical changes, although they may not be directly perceptible, *e. g.* when the lumines-

* Wied. Ann. xviii. p. 311 (1883).

cence is restricted to rays of greater or smaller wave-length than those which the eye can perceive.

Besides these oscillatory internal motions, of other temperature than the mean, which produce this luminescence, yet others may occur, rotatory and others, which are not of a nature to produce light-waves in the surrounding æther, as may be the case in the conduction of electricity through electrolytes. But of this I make here only a preliminary mention.

Luminescence and Kirchhoff's Law.

8. The production of light may therefore occur in consequence of a rise in temperature, as well as in consequence of a rise in luminescence. But these two modes must always be considered separately if we wish to obtain an insight into the mechanics of luminosity.

For luminosity resulting from a rise of temperature Kirchhoff's Law as to the relationship of emission and absorption holds good. Upon this rests the well-known reversal of the lines of the spectrum. The light produced by luminescence does not obey the same law, as is shown for example by the behaviour of fluorescent substances, which emit light of a refrangibility different from that of the incident light. In trying whether Kirchhoff's Law holds good or not, we are often able to distinguish the two phenomena. (See further on under 30.)

To discover the reasons why in glowing bodies Kirchhoff's Law of the ratio between emission and absorption holds good, and why this is not generally the case with luminescent bodies, let us consider the following circumstances :—

As we have said, there takes place in a gas a constant exchange between intermolecular energy and that due to motions of translation in consequence of the impact of molecules (the luminous energy forming a part of the intermolecular energy), so that a mean condition ensues. If any molecule suffers an increase of intermolecular energy, *e. g.* in consequence of the absorption of incident light, this is given up again in the next impact or so, and if it has a deficiency in intermolecular energy this is made good. The emission-coefficient depends upon the ease with which part of this intermolecular energy produced by the impacts, corresponding to the luminous energy, is given up again in the form of *light-vibrations*, that is upon the friction between the vibrating molecules of the body and the surrounding æther. The absorption depends upon the same quantity, and thus also upon the structure of the molecule. But since,

on the one hand, the coefficient of emission is greater the greater the friction, and on the other hand the coefficient of absorption equally increases with this; the coefficients of absorption and emission must run together; and so for all bodies in which this reciprocity exists Kirchhoff's Law must hold good.

The applicability of Kirchhoff's Law to the phenomena of luminosity thus assumes a uniform transmutability of luminous energy with that of translation, and *vice versâ*, for only in this case can the ratio between absorbed and emitted energy be a function of the wave-length. But if the conditions are such that intermolecular energies produced by the absorption of incident light &c., are not converted back into motions of translation after a few impacts, then the store of luminous energy will gradually increase, and a new emission will be added to that dependent on the temperature—that is to say, luminescence is produced. That Kirchhoff's Law no longer holds good here, and cannot hold good, is clear, since the structure of the molecule is such that the uniform transformation of luminous energy into that of motion of translation, and of that of translation into luminous energy no longer takes place. Indeed, it would seem as if Kirchhoff's Law only held good for an ideal case, viz. only if no increase of the luminous motions could be produced in the luminous body by absorption. Hence Kirchhoff's Law can only hold good for that part of the luminous motion which does not consist in luminescence. Moreover, Kirchhoff's Law has not been quantitatively proved for luminous gases; but we have contented ourselves with verifying certain qualitative-quantitative consequences of it.

Dependence of Luminescence on the Mode of Excitation.

9. Luminescent light is in a high degree dependent in colour and intensity upon the mode of production, so that in investigating it it is necessary to consider both of these qualities. In photo-luminescence, and so in fluorescence and phosphorescence, the colour of the emitted light is dependent upon that of the incident light. In electro-luminescence, discharges of various strengths call forth different assemblages of rays. The borders of the stratifications in discharge-tubes, as is well known, are of different colours towards the positive and negative poles. This occurs with hydrogen or air alone, but, as I have observed, more distinctly if the discharge-tube contains hydrogen and sodium vapour. Further, the glow-light and the positive light are, *cæteris paribus*, differently coloured.

The sulphur compounds of the metals of the alkaline earths, glowing in consequence of chemi-luminescence, yield light of different colours according to difference of temperature.

The order of intensity of emission of light may be completely reversed by change in the mode of excitation. Thus if, in electro-luminescence, a body A shines as bright as, or brighter than, another body B, this will not necessarily be the case upon ignition or with chemi-luminescence. Mercury and sodium give us examples of this. The first, introduced in the gaseous state into the flame, gives scarcely any light, whilst in a Geissler's tube it gives an intensely bright light; sodium, on the other hand, is very bright in both cases.

10. In many cases luminescence and ignition occur together. If we wish to arrive at conclusions in such cases we must endeavour to separate the two phenomena. The following are probably processes in which both phenomena occur together:—

In flame the production of light depends partly upon chemi-luminescence and partly upon phenomena of ignition, so soon at least as solid particles are separated.

If electric discharges pass between metal electrodes the metal is disintegrated and volatilized, and the vapour is heated to incandescence, at the same time it may be brought to luminescence by the electric current.

If we pass through a tube filled with hydrogen discharges of such a strength that the line-spectrum just appears, the hydrogen is far from being heated to the temperature of incandescence. If, on the other hand, we employ very strong discharges, we have, in addition to the original luminosity, a very great rise in temperature, which produces incandescence. We can expect to obtain an absorption corresponding to Kirchhoff's law only for the rays emitted by the process of incandescence, but not for the others.

The phenomena of long and short lines observed when an image of a horizontal flame is thrown upon the vertical slit of a spectroscope depend, no doubt, in part at least, upon the different processes of luminescence and incandescence. Several factors are simultaneously concerned in their production. Thus, for example, the short lines appear in the inner portions, and the long lines both in the inner and outer portions of the flame. But in the inner portions, according to the usual arrangement of the experiment, both the temperature and the quantity of incandescent material are greater than in the external portion. Experiments are in progress to separate these different conditions; this is especially important in order to decide which lines in the spectrum are produced by luminescence and which by incandescence; and how these

are arranged, as well as on account of their application to the (iron) lines of the solar spectrum.

Differences in the Mode of Evolution of Light, and of Emission of Light. Continuous and Discontinuous Excitation. Store of Luminous Energy.

11. In all investigations on luminosity two chief classes of phenomena are sharply to be distinguished: first, those where it is *always the same* particles which emit the light; and, secondly, those where *continually new* molecules take up the luminous motions.

The first is the case in the usual phenomena of luminosity, fluorescence, electro-luminescence, &c.; the second case occurs when the luminosity is produced by chemical changes, *e. g.* combustion, oxidation of phosphorus, of lophine, crystallization of arsenious acid, and, as I shall show further on, also in the luminous phenomena of phosphorescent calcium-sulphide compounds.

In the present treatise only the phenomena of the first class will be treated at length.

12. In considering the mechanics of luminosity, we must observe that there are two factors to be considered together.

First, a definite amount of energy must be communicated to the molecules, which gives rise to the production of the luminous motions; and, secondly, in consequence of the radiation of light a continuous diminution of this energy is brought about. The final condition of the body, as far as the phenomena of light are concerned, depends on the relation between these two quantities. A stationary condition ensues when the supply of energy is equal to the loss of energy.

(a) The *addition of energy producing light* may either be continuous, or it may be repeated after longer or shorter intervals.

A *continuous* addition occurs when a body is brought to photo-luminescence by means of incident light. The interruptions observed in the phosphoroscope are of course not to be taken into account, since in comparison with the vibration-period of light they are infinitely long. To the same category probably belong electro- and kathode-luminescence; since in the anode light the changes in the dielectric polarization of the molecules produce vibrations, whilst the kathode-rays are probably connected with the ultra-violet rays*.

A *discontinuous* excitation occurs in many other cases, as in glowing gases. In the contact of two molecules a part of the energy of translation is converted into luminous energy,

* Wied. Ann. xx. p. 781 (1883).

a part of which is lost by external radiation on the free path between two impacts.

The two cases require separate consideration, for the mode of excitation is essentially different. In incandescence it is the reciprocal relationships between the impinging molecules, whether of the same, or of different kinds, which produce the motions causing light; while in photo- and electro-luminescence these are due to an external motion affecting the particles.

(b) *The loss in luminous energy* may also have various causes. A loss of energy ensues in consequence of the issuing light-vibrations; further, in luminescent bodies in consequence of the impact of two molecules a part of the energy of the luminous motions may be converted into energy of translation and thus produce a rise of temperature. Further, within each molecule only those atoms associated in a particular way, the chromogenic, which we will here call *lucigenic*, may perform luminous vibrations together. These motions may in part be transferred to the neighbouring non-lucigenic atoms, and may thus suffer diminution. According to the structure of the molecules, only a particular kind of vibrations will be checked in any high degree, whilst others will remain undisturbed—a process to which numerous analogies are well known in acoustics.

We have appropriate examples amongst the phenomena of light in the observations on solutions of fluorescein and eosin in gelatine, which I have previously communicated*, where the gelatine is mixed with solutions of the substances and allowed to dry. In the fluorescent light yielded by these substances, observed while illuminated, the spectrum appears almost continuous from red to green. On the other hand, the phosphorescent light observed some time after the illumination has ceased, shows a very dark band in the orange. The phosphorescent light was examined in the phosphoroscope described by me, the arrangement being such that the phosphorescent light was examined from the same side as that on which the incident light fell†. The absorption of the light excited before the observation was diminished as much as possible. We must conclude from these observations that in these bodies the loss of luminous energy for the complex of rays in the orange is determined, not only by radiation, but also by an absorption within the molecule itself.

13. We will now consider the intensity-relationships of the light emitted by a body, and will investigate the two cases:

* *Sitzungsber. d. physikal-med. Soc. Erlangen*, July 1887.

† *Wied. Ann.* xxxiv. p. 453 (1888).

first, that the body is continuously excited ; and, secondly, that at any time the exciting cause is removed, and then the body, left to itself, gradually radiates its store of luminous energy.

A. If the body is continuously excited we may use the equation

$$di = \{\phi - bi\} dt. \quad . \quad . \quad . \quad . \quad . \quad (1)$$

The change of intensity di which occurs in the element of time dt is equal to the change of intensity ϕdt produced by the external cause, diminished by the change of intensity $bi dt$ in consequence of radiation, where we assume that the decrease in intensity is proportional to the intensity existing at the moment*.

b is, as follows from the equation, the reciprocal value of the time in which the unit of intensity is radiated if the radiating body is maintained at unit brightness. The decrease in brightness may be produced both by radiation and by internal absorption.

The function ϕ is essentially different according to the mode of excitation.

For phenomena of photo-luminescence we may assume that $\phi = AJ$, *i. e.* that ϕ is proportional to the intensity J of the incident light. A is the reciprocal value of the time necessary for unit intensity to be excited by incident intensity 1. We may also say that A expresses how large a fraction of the incident intensity is converted into excited intensity in unit time. Then

$$di = (AJ - bi)dt.$$

Hence, if C is a constant,

$$i = \frac{1}{b} (AJ - Ce^{-bt})$$

If $i=0$, for $t=0$, then $C = AJ$, and

$$i = \frac{AJ}{b} (1 - e^{-bt}). \quad . \quad . \quad . \quad . \quad . \quad (2)$$

If we make some other assumption for the relationship between the decrease in intensity di and the intensity i , equa-

* This equation holds good in the first instance for the communicated and radiated intensities ; but if we assume that the radiated intensity is proportional to the luminous energy existing at the moment, it may further be applied without alteration to the intensities of the luminous motions.

tion (1) and consequently equation (2) will take a different form; but since with increasing intensity the quantity radiated must also increase, we shall have an equation analogous to (2). The further conclusions will therefore not be essentially affected.

Strictly speaking a separate equation of the form of (1) is required for rays of each wave-length. For the value of b may be very different for the rays of different wave-length emitted, as the experiments described above with eosin and fluorescein show; and, again, the value of A is very different for exciting light of different colours, as we learn from numerous experiments with fluorescent substances.

The intensity i_f of the *fluorescent light*, i. e. the intensity of the light observed upon continuous illumination, is determined by the value of i for $t = \infty$. It is so great that the loss is equal to the gain; hence $di/dt = 0$. Hence it follows that

$$i_f = \frac{AJ}{b} \cdot \cdot \cdot \cdot \cdot \cdot \cdot (3)$$

From this expression we see that the brightness of the fluorescent light depends upon two quantities—first, on the fraction of the incident energy converted into light-vibrations; and, secondly, on the loss of energy determined by b . The first quantity is dependent upon absorption &c.; but the second upon the strength of the emission and the loss of energy either in the impact of two molecules or in consequence of mutual attractions of the constituent atoms of a molecule.

The great increase in intensity of the fluorescent light, which occurs when solutions of fluorescent substances are thickened by addition of glycerine, gelatine, &c., may be referred to the decrease in that portion of b which corresponds to the loss of energy by mutual impacts, since, in consequence of the greater friction and consequent less mobility, the molecules of the same kind impinge much less frequently.

In other cases we may find in solutions of the same substance in different media hardly any difference in the intensity of the absorption, but a displacement of the absorption-bands, as with safranine and magdala-red, and at the same time a complete disappearance of fluorescence. The explanation in my opinion is to be sought in the formation of hydrates &c., and a consequent alteration of absorption-relationship within each molecule in a way as

yet unmeasurable. F. Stenger has published another explanation*.

B. Let us now turn to the second case. Let the luminous energy be brought by any cause to a constant height, and then at a time $t=0$ let the radiating body be left to itself, after the exciting cause has been removed. We will further assume that the loss of energy takes place by radiation and not by absorption. Further, the luminous energy contained in a particle shall not receive any further increase during the radiation, in consequence of processes going on within the molecule itself, or by the impact of two molecules.

If, then, i is the intensity measured in any units, *i. e.* the energy emitted in the unit time at the time t , and b the constant introduced above, then during the time dt the radiating body suffers a loss of luminous energy

$$di = -bidt.$$

If we integrate this expression from 0 to ∞ we obtain the total store of energy of the vibrating particles, for in an infinitely long time all the energy will be radiated; hence the total luminous energy present is

$$L = \int_0^{\infty} i dt = \int_0^{\infty} i_0 e^{-bt} dt = \frac{i_0}{b}.$$

If, then, we know the intensity i_0 at the time 0, and the constant b , we can find the total luminous energy contained in the luminous body under the above assumptions. *The store of luminous energy is equal to i_0 , the initial intensity, divided by b , the constant of loss of energy.*

Total and True Coefficient of Emission.

14. We may express the energy emitted by the unit weight of a body in the unit time contained in the rays lying within an infinitely small breadth of the spectrum between wave-length λ and $\lambda + d\lambda$ by $s_\lambda d\lambda$; s_λ would then denote the energy contained in the region between λ and $\lambda + 1$ if at all points within the same the same energy is yielded as at the point λ ; we may appropriately call s_λ the true coefficient of emission at the point λ , referring, of course, to the unit weight. The radiating layer is supposed to be so thin that the absorption of the emitted rays within it may be neglected. The energy is to be measured in calorimetric units. If the region of the spectrum that we are considering extends from λ_1 to λ_2 , the

* Wied. Ann. xxxiii. p. 577 (1888).

energy emitted is

$$S_{\lambda} = \int_{\lambda_1}^{\lambda_2} s_{\lambda} d\lambda.$$

The quantity S_{λ} we will designate as the *total coefficient of emission* of the unit weight between the wave-lengths λ_1 and λ_2 . It is the energy emitted in the unit time by the unit weight of the body in question corresponding to all rays between the wave-lengths λ_1 and λ_2 . The two quantities S_{λ} and s_{λ} are exactly analogous to the total quantity of heat necessary to heat a body from t_1° to t_2° and the true specific heat. The two quantities S_{λ} and s_{λ} in the form given above have not been experimentally determined as yet. We have above all not referred the emission to a definite quantity of the radiating body, but only to the unit of surface of the particular body. The molecular coefficients of emission are obtained by multiplying s_{λ} and S_{λ} by the molecular weight of the body under investigation.

15. In this investigation of spectra two problems may occur. We determine

(1) The *total coefficient of emission* $S_{\lambda_1\lambda_2}$ between the wave-lengths λ_1 and λ_2 of a body which is maintained in a constant condition (*e. g.* of a platinum wire of constant temperature). Then the total coefficient of emission can be determined for the whole spectrum from $\lambda=0$ to $\lambda=\infty$, or for particular parts of it, which may ultimately consist of one or more so-called spectral lines or bands stretching continuously between every two wave-lengths. Then S assumes the value

$$S_0^{\infty} = \int_0^{\infty} s_{\lambda} d\lambda \text{ and } S = \int_{\lambda_1}^{\lambda_2} s_{\lambda} d\lambda + \int_{\lambda_3}^{\lambda_4} s_{\lambda} d\lambda + \dots$$

It is to be observed that the value of the first integral cannot be directly determined, since we do not know what the radiation is either for very small wave-lengths or for very great wave-lengths, but our experiments are limited to a very small portion of the possible rays. Further, we must observe that in our experiments as soon as s_{λ} relates to rays which are also given off by surrounding bodies, we determine not the coefficient itself but $s_{\lambda} - a_{\lambda}$, where a_{λ} denotes the coefficient of emission of the bodies serving for the measurement of the wave-length λ , also in calorimetric measure.

(2) We determine the *true coefficient of emission* s_{λ} for a *single* wave-length if belonging to a definite point of the spectrum. Here we must observe that line-like portions of the spectrum are not directly comparable with continuous

spectra, but account must be taken of dispersion (see further on).

16. For the experimental determination of the two coefficients of emission in the visible spectrum measured in calorimetric measure, we must determine for a definite body, best a perfectly black one, the radiation in calorimetric measure, and compare (by the method to be explained immediately) with its brightness that of the body to be investigated, by making the brightness of the two bodies exactly the same, since with equal brightness the energies of the rays perceived by the eye in the same regions of the spectrum are equal. We thus fix the ratio of the energies received by the apparatus; these are themselves proportional to the coefficients of emission of the two sources of light, and are further dependent upon the distance of the luminous body, the thickness of the radiating layer, &c. (see further on). Further, the dispersion must be taken account of.

[To be continued.]

XXV. *Note on the Thermoelectric Position of Platinoid.* By J. T. BOTTOMLEY, M.A., F.R.S., F.C.S., and A. TANAKADATÉ, *Rigakusi**.

IN carrying out a series of experiments on radiation of heat by solid bodies, an investigation to which one of the present writers has for some time past devoted considerable attention, it became necessary, for a purpose which need not here be detailed, to select a thermoelectric pair of metals, of which one metal was essentially platinum, as it had to pass through glass. Various pairs were considered, and some trials were made; and it was finally determined to make use of platinum and platinoid. The latter metal is an alloy whose electrical and mechanical properties were investigated some years ago by one of the present writers†; and since that time it has assumed considerable importance in the construction of electrical instruments and resistance-coils. Partly on this account, and partly from present requirements, it became both interesting and necessary to determine the thermoelectric constants for a specimen of this alloy.

Platinoid is in composition very similar to German silver. In the manufacture of the alloy, however, phosphide of tungsten is employed; and although an exceedingly minute

* Reprinted at the request of the Authors, having been read before the Royal Society, June 20, 1889.

† J. T. Bottomley, Roy. Soc. Proc. 1885.

quantity of metallic tungsten remains in the alloy, yet the properties of the substance are in many respects remarkable. The metal is capable of being polished so as to be almost as beautiful as silver in appearance, having only a slightly darker and more steel-like colour; and when it has been polished it remains absolutely untarnished even in the atmosphere of a large town, for years at any rate. It has very remarkable properties as to electric resistance. It possesses a very high resistance, while at the same time it has a much lower temperature-variation of electric resistance than any other known metal or alloy. It has also, as Sir William Thomson has found, very excellent elastic qualities.

Although it was not proposed to use the platinoid with any metal other than platinum in the investigation on thermal radiation above referred to, it nevertheless seemed advisable, when these experiments were being undertaken, to determine its position with respect to some other metals. It was accordingly tried as a pair with platinum, iron, aluminium, and with two specimens of copper.

A low-resistance Thomson's reflecting-galvanometer was specially prepared for the purpose of the experiments. The mirror was a plane parallel mirror of very excellent quality, by Steinheil of Munich. Its deflexions were observed by means of a telescope with cross-wires and scale, instead of with lamp and scale. To avoid any influence of the suspending fibre (which even though of single cocoon-silk fibre does with short fibres give an appreciable torsional resistance) the mirror was suspended by spider-line. The suspending of a mirror, weighing with its magnet 0.2 gram, by a single spider-line is a matter of some nicety and difficulty; but when it has been accomplished the result is so thoroughly satisfactory that it is easily admitted to be well worth a morning's labour.

To make the suspension, two small pieces of very thin bristle or of hard-spun silk fibre or split horsehair are attached to the ends of a suitable length of spider-line recently spun by a good large spider*. By means of these attachments, which are easily seen, the spider-line can be handled. It is then brought over the galvanometer-mirror; and great assistance is experienced in these operations, and in operations with single silk fibres, by performing them on the top of a piece of looking-glass laid on the table. The illumination from beneath of the fibres makes it easy to do with these fine filaments that which is otherwise scarcely possible. The fibre is attached to the

* The body about as large as a pea.

galvanometer-mirror with the smallest possible speck of shellac varnish, the greatest care being taken not to varnish any part of the spider-line. When the varnish has dried, the mirror can be lifted up by the spider-line; caution being used at the moment of raising the one mirror off the surface of the other, on account of the vacuum which is liable to be formed at the moment of separation. The mirror should be allowed to hang on the fibre inside a glass beaker for twenty-four hours at least, as the spider-line stretches considerably for some time after the weight comes on it. A spider-line which will carry a galvanometer-mirror and magnet weighing 0.2 gram may have, according to an estimate made by one of the present writers, about $\frac{1}{700}$ of the torsional rigidity of a single cocoon-silk fibre.

For the heating of the junctions, a number of glass vessels were blown, resembling the flasks, with neck and condensing-tube, used for fractional distillation, but with the condensing-tube projecting upwards into the air, so that the steam of a liquid boiling in the flask ran back into the flask on being condensed. Into the shorter neck of the flask was introduced a cork, which carried the thermo-junction and a mercurial thermometer; the thermo-junction being loosely bound to the bulb of the thermometer, or, at any rate, kept in close contact with the middle part of the thermometer-bulb. The cool junction was bound to the bulb of a second thermometer, which dipped into a vessel containing water at the temperature of the laboratory. The water was kept thoroughly stirred from top to bottom by a properly arranged stirrer.

In the heating-flasks the vapours of the following liquids were used:—alcohol, water, chlorobenzol, aniline, methyl salicylate, and bromobenzol*. The liquids were boiled vigorously, and the temperatures of the vapours were determined by means of the mercurial thermometer. Both the mercurial thermometers were compared directly with the air-thermometer†. The obtaining of a set of points of temperature by this means was very satisfactory in every case except that of the liquid of highest boiling-point—bromobenzol. In this case a curious phenomenon was observed‡. In spite of the fact that the vapour of the substance was rushing strongly into the condensing-tube, and, indeed, out into the open air at an elevation of two feet above the surface of the liquid, it was found exceedingly difficult to keep the temperature of the various parts of the boiling flask anything like uniform. The

* Ramsay and Young, *Chem. Soc. Journ. (Trans.)*, 1885.

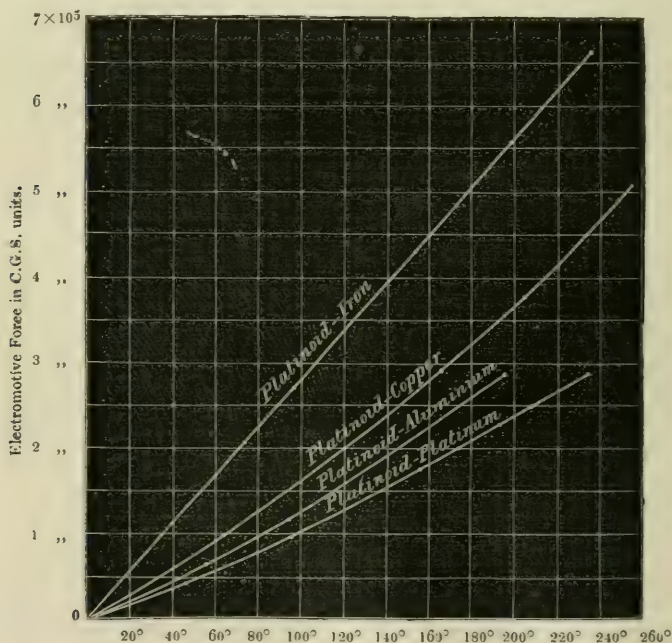
† J. T. Bottomley, *Phil. Mag.* August 1888.

‡ Perhaps due to want of purity of the substance.

vapour formed itself into layers of different temperatures, the parts of the flask nearest the surface of the liquid being the hottest. At a height of $2\frac{1}{2}$ inches above the surface of the liquid the temperature was often found to be as much as 8° or 10° C. cooler than it was just above the surface. The difficulty could, to a certain extent, be overcome by putting a cloak of fine flexible wire gauze all round the upper part of the flask; but the greatest watchfulness was needed to avoid mistakes.

In order to reduce the results obtained from the readings of the galvanometer to absolute electromagnetic measure, a carefully prepared standard Daniell's cell was kept with its current always flowing through a known high resistance; and from time to time the galvanometer which was being used was thrown into the circuit, and the value of the galvanometer-deflexion determined. The electromotive force of the Daniell's cell was valued at 1.072 volt.

The results obtained are shown in the accompanying curves and tables.



In the curves the electromotive forces are shown as ordinates, the differences of temperature between the hot and cold

junctions being indicated on the axis of abscissæ. The electromotive forces are given in C.G.S. units, and must be divided by 10^8 if it be desired to reduce them to volts. The differences of temperatures are given in Centigrade degrees. The direction of the current in each of the cases represented is from platinoid to the second metal of the pair through the hot junction.

Table I. shows, in the way now well known*, the multiplier at any temperature Centigrade which must be used as factor with the difference of temperatures between the hot and cold junctions, in order to calculate the electromotive force in C.G.S. units. The algebraic sign corresponds with that used by Tait, and now adopted by Everett ('Units and Constants,' second edition, 1886).

TABLE I.

Platinoid-platinum	— $925 - 1.16 \times t$.
Platinoid-aluminium	— $985 - 4.31 \times t$.
Platinoid-iron	— $2916 + 0.86 \times t$.
Platinoid-copper (A)	— $1246 - 5.44 \times t$.
Platinoid-copper (B)	— $1294 - 4.88 \times t$.

Combining the results of Table I. with those of Tait, reduced by Everett, we obtain the thermoelectric distance of platinoid from lead, taken as zero, at various temperatures Centigrade. If any one of the wires platinum, aluminium, iron, or copper used by us were identical with the wire of the same name used by Professor Tait, we should be able to deduce with exactness the distance of our platinoid wire from his lead wire. This, however, was not the case; and each of the secondary wires used by us gives us, as it were, a different result. Thus we have:—

TABLE II.

		Platinoid to lead.
From experiment with platinum . .		— $986 - 2.26 \times t$.
" " aluminium . .		— $1062 - 3.92 \times t$.
" " iron		— $1182 - 4.01 \times t$.
" " copper (A) . .		— $1110 - 4.49 \times t$.
" " copper (B) . .		— $1158 - 3.93 \times t$.

Taking the mean of all of these, with the exception of the result for platinum, which we omit because different specimens of platinum are well known to differ thermoelectrically enormously among themselves, we obtain for the thermo-

* Tait, Edinburgh Roy. Soc. Trans, vol. xxvii. (1873), and Everett's 'Units and Constants,' second edition, 1886—"Thermoelectricity."

electric distance of platinoid from Professor Tait's lead wire $-1128-4.1 \times t$.

This result enables us to place platinoid in Tait's thermoelectric diagram. Its line is nearly parallel to those of palladium and German silver, and slightly above the latter. It is, however, to be remembered that, in all probability, different specimens of platinoid alloy would give results differing considerably from that quoted above.

Appendix. By A. TANAKADATÉ.

The following experiment on the torsional rigidity of spider-line was carried out in the Physical Laboratory of the Imperial University of Japan in 1884, and a notice of it was published in vol. ii. of *Rigakukyokwai Zasshi* ('Proceedings of the Science Society') of that year in Japanese. It has not hitherto been described in English; and the absolute determination, as referred to below by Mr. T. Gray, of the rigidity of silk fibre makes an estimate of the rigidity of spider-line possible.

The determination of the torsional rigidity was a relative one, and the experiment essentially consisted in finding the deflexion of a small magnet due to a given twist of the suspending fibre; the magnet being placed in the earth's magnetic field (0.3 C.G.S.). The deflexion was observed by the usual method of the reflected image of a fine wire stretched before a lamp.

The mirror-magnet was first hung by a silk fibre of 31 centim. length and placed in the usual way. The distance of the scale from the mirror was 95 centim. When the torsion-head of the magnetometer was turned through one complete revolution (2π) in either direction from zero, the image of the reflected wire was displaced through 8 millim. either way, or $\cdot 8/2 \times 95 = 0.0042$ radians, or $864''$.

The silk fibre was now detached from the magnet and a spider's line (newly spun) was attached in its stead. The length was 28 centim., the magnetometer was put into its place, and the torsion-head was turned as before; but no appreciable deflexion could be observed, even when the torsion-head was turned through ten complete turns (20π). It was then suspected that the mirror might have been caught against the sides of its case; a close inspection, however, showed that it was quite free. The fibre was then shortened to 2.3 centim. (about one twelfth its previous length) and the experiment was repeated. Ten complete turns of the torsion-head gave a deflexion of 1.5 millim.; or $\cdot 15/2 \times 95 = 0.00079$ radians $= 16'' \cdot 3$ per turn.

In order to compare these deflexions with each other, each deflexion was reduced to that which would be given by a fibre of 1 centim. in length by multiplying the deflexions by the length of the fibre used. Thus, corresponding to the twist of one turn of the torsion-head in a fibre of 1 centim. long, we have :—

For silk fibre . . . $864''\cdot0 \times 31 = 26800''$

For spider-line . . . $\frac{16''\cdot3 \times 2\cdot3}{10} = 37''\cdot5.$

From this we get the ratio of the torsional rigidity of the spider-line to that of the silk fibre to be 1 : 710.

The diameters of the fibres were microscopically measured, and gave the following values :—

Silk fibre 0·00091 centim.

Spider-line 0·00028 „

If the elastic qualities of these fibres were the same, the ratio of the torsional rigidity would have come out

$$(28)^4 : (91)^4 \text{ or } 1 : 112 ;$$

and hence the torsional rigidity of spider-line is less than one sixth of that of silk fibre of the same thickness.

The above result gives us only a relative value of the rigidities between the two fibres. If we take the mean value of the torsional rigidity of silk fibre to be 0·0012 C.G.S. on a length of 1 centimetre (not per square centimetre), as found by Mr. T. Gray*, the torsional rigidity of the spider-fibre of the above experiment will be $\frac{0\cdot0012}{710} = 0\cdot000002$ C.G.S., the mode of reckoning being the same.

Mr. Gray's silk fibre may have had a slightly higher rigidity, as he states that it was boiled in water, while the fibre of the experiment just described was taken from those boiled in dilute potash water, as is the usual practice of preparing "mawata," which is a very soft kind of silk.

XXVI. On the Theory of Hail.

By CHARLES TOMLINSON, F.R.S., F.C.S., &c.†

[Plate V.]

FROM the time of Franklin even to the present day an article in the orthodox meteorological creed requires us to believe that the formation of hail is due to electrical action. It is true that nonconformists have appeared from time to time,

* Phil. Mag. Jan. 1887.

† Communicated by the Author.

but not in sufficient number to form a sect; and also indifferentists, who say that the two former classes quarrel about what no one understands. Thus Kämtz refers to hail as a phenomenon “welches sehr häufig Begleiter der Gewitter ist, und dessen Erklärung sehr schwierig ist.” Nevertheless his elaborate treatment of this subject is included in the chapter on Atmospheric Electricity¹. Pouillet more emphatically declares:—“On ne sait rien jusqu'à présent sur les causes qui déterminent ce phénomène”². Becquerel also confirms the remark:—“Nous ignorons encore quelle est la véritable origine de ce phénomène”³.

Yet there is something very enticing in the electrical theory of hail. Hail often accompanies the thunderstorm; and the stones, in falling, have been known to flash with electric light⁴. It is true that in the last century, when Franklin's experiments were exciting delight and enquiry in various parts of Europe, more burdens were laid on electricity than it was well able to bear; and we may perhaps allow a good-natured smile to pass in reading Father Beccaria's statement that “a more intense electricity unites the particles of hail more closely than the more moderate electricity does those of snow”⁵. M. de Morveau also supposes that, evaporation being the immediate cause of the cooling, and electricity augmenting perceptibly the evaporation, “le fluide électrique est une cause habituelle plus ou moins immédiate de la formation de la grêle”⁶.

Without referring further to the ideas of the older physicists as to the formation of hail, I pass on to a meteorologist nearer our own time, who exerted considerable influence on the science of meteorology—if such it can even now be called. Professor Daniell⁷ admits the difficulty of accounting for the phenomena of hail on account of our imperfect knowledge of the influence of electricity. As a proof of electrical action, he calls attention to the behaviour of the electrometer on the approach of hail, when the electricity will not only be found frequently to change in intensity, but also to pass from positive to negative, and *vice versâ*, ten or twelve times in a minute.

From this action on the electrometer Kämtz was led to include hail under atmospheric electricity. Even the small hail that falls in winter (which the Germans name *Graupel*, and

¹ *Lehrbuch der Meteorologie*, ii. p. 495.

² *Elémens de Physique et de Météorologie*.

³ *Traité de l'Électricité*, iv. p. 151.

⁴ Lauke Howard, ‘Climate of London,’ cites examples of luminous hail.

⁵ Quoted in Priestley's ‘History of Electricity.’

⁶ Bertholom, *De l'Electricité des Météores*, ii. p. 189. This was at one time a work of considerable authority.

⁷ *Elements of Meteorology*, 3rd edition, 1845, i. p. 241.

the French *grésil*), and does not accompany a thunderstorm, he classes with the hail, properly so called, that often does do so. He regards both kinds as electrical phenomena, only the electricity has more tension in the one case than in the other. But when a man is on a wrong tack, he is sure to meet with facts that contradict his hypothesis. In such cases Kämtz's aphorism will apply to many an observer :—" Man glaubt dennoch was man gern wünscht ;" but it does not apply to Kämtz himself, for he honestly states facts that are against him, and waits for further light. Thus he has occasionally noticed larger hail-stones in winter than in summer, although there is more moisture in the air in the latter season than in the former. Smaller hail is found on the top of the mountain than in the valley below, just as if the increase in volume took place during the fall. The inhabitants of mountain districts speak of *Graupel*, whilst those in the valley refer to it as *Hagel*.

The advocates of the electric theory of hail had no better method of accounting for the large masses that fell, consisting often of a number of coatings round a nucleus, than by supposing that a small stone gathered to itself clear and opaque coatings of ice during its descent. This idea prevailed long after Volta attempted to explain the formation by a well-known electrical process. Mrs. Somerville⁸ adopts this view; and Prout⁹ supposes that there must be formed an icy nucleus far below the freezing-point, acquiring magnitude as it descends by condensing on its surface the vapour of the lower regions of the atmosphere.

As the formation of hail was firmly believed to be an effect of the sudden disturbance of the electrical equilibrium of the clouds, it was supposed that if the electric fluid could be quietly and gradually drawn away hail would be altogether prevented. Accordingly, soon after the introduction of the lightning-conductor (*paratonnerre*), it was proposed in France, where hail is regarded as a real scourge, to introduce a hail-conductor (*paragrêle*)¹⁰. For this purpose, tall wooden poles were erected, furnished at the top with a sharp copper point, and connected by means of a metal wire with the ground. In some cases the wire was omitted, but as wood is a bad conductor it is difficult to see the use of the poles (even supposing the theory that erected them to be valid). It was even contended that the poles were equally efficacious with or without the wires; and this we can readily believe, seeing that a tree ought to be more efficacious than the pole, in consequence of its greater elevation and the multitude of points presented by its

⁸ Physical Geography.

⁹ Bridgewater Treatise.

¹⁰ *Journal de Physique*, 1776.

twigs and leaves. Nevertheless, hail was known to be of frequent occurrence in well-wooded localities, also in towns where lightning-conductors were common. In spite of this, vast numbers of poles, with or without metal wires, were erected at great cost in fields and vineyards in various parts of Europe; and in 1820 an ignorant apothecary recommended pillars of straw as being excellent *paragrêles*, and they, too, were extensively adopted. Well may Becquerel denounce the *paragrêle* as “cette invention de l'ignorance dont la science et le bon sens public ont déjà fait justice”!

Other methods of guarding against hail have been recommended and adopted, such as making fires on the ground on the approach of a storm, discharging artillery and otherwise exploding gunpowder; but these methods must be classed among the inventions of ignorance, although so good an observer as Matteucci¹¹ refers to a village in Italy where the peasants, acting under the advice of the Curé, place, at intervals of about 50 feet, heaps of stones and brushwood, and set fire to the latter when a storm is seen to be approaching. The plan had only been adopted three years when Matteucci made his report, and that is too short a time to base any conclusion on; but it has been suggested that, while hail does great damage in the outskirts of London, it is less harmful in the denser part of the metropolis, probably on account of the vast column of heated air that rises from it altering the local atmospheric conditions required for the production of large hail.

The occurrence of hail in hot weather, often at the hottest part of the day, sometimes in the form of masses of ice of considerable size, apparently from a cloud situate far below the snow-line, is a problem that has often appealed to the scientific ingenuity of physicists for solution. As already remarked, the accompanying electrical displays naturally led to an electrical theory, of which Muschenbroek¹² was the original parent. He was succeeded, amongst others, by Mongez¹³, Muncke¹⁴, De Luc¹⁵, Lichtenberg¹⁶, Lampadius¹⁷, and lastly by Volta¹⁸. Now, as the inventor of the pile and of the electrophorus took higher rank as an electrician than the others just named (whose hypotheses he doubtless had examined), it will be

¹¹ Arago, *Meteorological Essays, Sur la Grêle.*

¹² Introduction.

¹³ *Journal de Physique*, vii. p. 202.

¹⁴ Gehler's *Wörterbuch*, v. p. 54.

¹⁵ *Idées*, ii. sec. iii. chap. 2.

¹⁶ *Schriften*, viii. p. 85.

¹⁷ *Atmosphärol*, p. 153.

¹⁸ *Sopra la Grandine, Opere*, i. part ii. p. 353.

sufficient for our present purpose to give an outline of his theory.

This theory has first to account for a reduction of temperature sufficient to freeze water on a hot day, and then to explain how the hailstones, so formed, can be held suspended in the air so as to attain a volume often of several inches in circumference. The cold is supposed to be produced by a powerful evaporation, due to the action of the sun on the upper surface of the cloud; and the evaporation is the more rapid in proportion as the air above the cloud is rarefied and electric, for it is admitted that electricity greatly favours evaporation. Hence one portion of the cloud, in evaporating, lowers the temperature of the other portion sufficiently to produce congelation.

The nuclei of the hailstones being thus formed, they cannot attain fresh coatings of clear transparent ice, however low in temperature they may be, in the short time they are falling to the earth; but it is in the power of electricity so to sustain them, while they are thus being developed in volume and weight.

Let us suppose that a cloud, strongly electrified, is suddenly congealed at its upper surface, in consequence of an energetic evaporation. The result will be a multitude of small frozen particles which form the nuclei of the hailstones. These particles, repelled upwards by the strong electric action of the cloud, are held suspended at a certain distance, just as a feather is when an excited glass tube is held under it. In like manner, if these frozen particles are placed on an insulated horizontal plane, and this be strongly electrified, they will rise up into the air and remain there so long as the plane retains its electricity, or until they lose their electric charge, when they will fall back by their weight upon the plane, and take a fresh charge and be again repelled. During these motions the hailstones increase in volume by condensing the vapour of water upon their surfaces, and this immediately becomes solid. A few of the stones increase in size more rapidly than the others, and these are the first to fall—the *avant couriers* of the general shower of hail when the weight of the individual stones is too great for the electric force to maintain them suspended.

But the action above described is more complete by supposing the existence of two or more clouds, one above the other, in opposite electrical states. In such case the motion of the frozen particles is much more rapid; it is like the pith figures oscillating between two metal plates in opposite elec-

trical states, when the attractions and repulsions are performed with great celerity, and the hailstones, being equally active, produce that peculiar noise that precedes a fall of hail¹⁹.

Volta's theory has frequently been discussed, and objected to on many grounds. Becquerel points out the mistaken notion that vapour in the act of forming becomes negative, and while being condensed positive. When a dense cloud is acted on by the sun, vapour arises charged with the same electricity as that of the cloud; but when, on reaching an upper and colder region, it becomes condensed, it is said to assume an opposite kind of electricity. But the clouds are not electrified after this manner, seeing that electricity is not set free by change of state, unaccompanied by decomposition. Moreover, the question arises why the two clouds in opposite states, connected as they are admitted to be by conducting particles of vapour, do not immediately neutralize each other. The oscillations of the hailstones between the two clouds could only take place if the clouds were solid planes, as in the experiment with the dancing pith figures. Then, again, the snowflakes which form the nuclei of the hailstones are said to be formed at the upper surface of the lower cloud, and hence must form a portion of it; how, then, can they be driven out of it without breaking up the whole cloud? Even supposing them to have reached the upper cloud, they must form an integral portion of it, and can escape from it only by their weight, and falling upon the moist surface below must be detained there, as in the dancing-figures experiment, if, instead of the lower metal plate, a surface of water be substituted, the adhesion between this and the pith figure is so great that further motion ceases.

It will be remarked that in Volta's theory the action of the sun in promoting the evaporation of the cloud is all important. This might be admitted if hailstorms occurred only by day; but such storms may occur at any hour of the night as well as of the day. Kämtz has collected a long list of nocturnal hail-

¹⁹ The sound that precedes the fall of hail is supposed to be due, not only to the rattling of the stones against one another, but also to the fierce wind from all quarters that usually accompanies a hailstorm. The sound has been variously compared to the rushing or roaring of waters, as when Morier, in Persia, thought that the river had suddenly swollen into a torrent. Kämtz likens the sound to the rattling of a large bunch of keys; Peltier to that of a flock of sheep galloping over a stony road; Daniell to the emptying of a bag of walnuts. Others speak of the noise as crackling, chattering, clashing; and Volta regarded it as one of the strong proofs of his theory. It should also be stated that the noise has been attributed to the combination of the individual sounds produced by each hailstone cutting the air with great swiftness.

storms, dating from the year 1449 to his own time. One example will suffice for our present purpose. At midnight, between the 25th and 26th June, 1822, a violent storm of hail burst over Meissen, and the next morning the farmers had to mourn the entire loss of their crops of fruit and grain, and they found hundreds of starlings lying dead in the fields.

Another objection with respect to the sun is that, if it promotes evaporation, it also raises the temperature, as was shown by Bellani²⁰, who covered the bulbs of two thermometers with wet linen, and exposed one to the sun and the other to the shade. Evaporation was the more rapid in the sunshine, but the temperature was higher; whereas, according to the theory, it ought to have been lower, even to freezing.

The celebrity of Volta's name gained for his theory much attention, and with some modifications it was more or less adopted. Perhaps the most distinguished physicist who quarrelled with it, and yet put forth a theory very much like it, was Peltier²¹, who, in announcing it, complacently remarks that "Volta a placé des suppositions où je place des faits."

Peltier also imagines two clouds in opposite electrical states, placed one above the other. Their mutual attraction is considerable; they approach without any notable discharge, but the electricities are exchanged, and there can be no such exchange without producing vaporization of the minute drops or vesicles which compose the clouds. Hence there is a lowering of temperature, rapid in proportion to the electric tension of the two clouds. Should the temperature of the clouds be considerable, no noticeable effect ensues; but if one be at or below the freezing-point, some portions of the cloud that had not been vaporized are converted into flakes of snow, which act as nuclei to the hail-stones. These flakes are quickly surrounded by condensed water, which freezes into transparent ice. The globules fall by their own weight from the upper to the lower cloud, where they become recharged and wetted. They are then attracted by the upper cloud, change their electricity, and become reduced in temperature by radiation and evaporation, and so acquire a new coating of moisture, which freezes. They again return to the lower cloud, and thus by a series of oscillations increase in volume until they become too heavy for the attractive force of the electricity, and they fall to the ground.

Many of the objections urged against Volta's theory apply also to Peltier's. But in matters of science the authority of a great name is so potent that a false theory stamped with it will retain its vitality long after its funeral obsequies have

²⁰ Brugnatelli's *Giornale*, x. p. 339.

²¹ *Météorologie*, chap. xvi.

been performed. Thus in 'Nature,' for June 13th last, an account is given of a hailstone that fell at Liverpool on the 2nd of that month at 3.35 p.m., consisting of an opaque nucleus surrounded by a circle of almost clear ice with fine circular lines, and that was bounded by a frilled outline of opaque ice. The writer goes on to say :—"If a hailstone is formed during electric oscillation from cloud to cloud, and if it receives opaque ice from one cloud and clear ice from another, the alternation of layers would be a natural consequence. The violence of the hail scarcely seemed as great as their size justified, and this suggested that electrostatic attraction had upheld them against the force of gravitation down to a moderate height above the ground."

Of course it is not meant to deny that in the formation and fall of hail two or more layers of cloud may exist in opposite states of electricity. All that modern theory contends for is that the electricity and the hail are not related as cause and effect. The hail-clouds do not often, if ever, assume the well-defined planes described in Volta's theory. Hail-clouds are generally very massive, of a peculiar ash-grey colour, very different from that of other clouds; the edges are much rent, and there are swellings and outgrowths on the surface. On some occasions the hail-cloud is made up of rounded clustered masses with long processes shooting downwards almost to the earth, before it discharges its icy load. Péron²² gives an account of a storm at Sydney, in Australia, in which there were several layers of cloud. In the morning the weather was fine and the sea tranquil, but soon after noon the wind suddenly veered to the N.W. blowing in squalls. An enormous mass of black cloud was driven by the wind from the summit of the Blue Mountain into the plain below, and it seemed so dense as to cover the face of the ground. The heat became overpowering, the thermometer rising suddenly from 73° to 95° F. The clouds burst open with a fearful noise, and a dazzling lightning of a bluish colour everywhere prevailed. The wind blew from all points of the compass, and its violence increased in proportion as the disorder and change in direction became more evident. Each time that a fall of heavy raindrops occurred, the end of the storm was looked for, but each time also there fell a copious hail from a cloud that was far higher and blacker than all the others.

M. Le Coq²³ describes a storm among the mountains of Auvergne, where the hailstones were mostly of the size of a

²² Voyage, i. p. 396.

²³ Quoted by M. de la Rive in a paper "On the Formation of Hail," Edinb. New Phil. Journ. xxi. p. 280.

pullet's egg, but some as large as a turkey's. There were two strata of clouds, one over the other, and two winds from different quarters, both which conditions he considers necessary for the production of hail. Early on the morning of July 28th, 1835, the sky was cloudless, but about 10 a.m. the heat became intense, and at noon almost intolerable. Thin flakes of vapour were seen floating at a great distance, the wind was N. but feeble. At 1 p.m. it had freshened, and white clouds had descended considerably, and soon after covered a great part of the horizon. They were of a greyish tint, which became darker and darker till nearly black, and at 2 p.m. they covered the whole of Auvergne. Flashes of lightning were seen, and a distant low murmuring sound was heard, when a vast cloud advanced from W. to E., pure white in some places, chiefly at the edges, and deep grey at the centre: it seemed to advance rapidly under the impulse of a violent west wind, and it sailed below all the other clouds; its borders were festooned and deeply slashed, and protuberances in the shape of long nipples were suspended from the lower portion. At 2.15 the cloud had approached nearer and the noise became very intense, the edges of the cloud seemed to be in rapid motion, and hail was apparently within it. Soon after this whirlwind kind of motion hail fell, and did much damage, it being propelled by the N. and the W. wind, it took the mean direction. The hailstones that now fell succeeded one another very slowly, but all at once there was an immense downpour. After this the distant rolling sound entirely ceased, and the cloud, freed from its swelling appendages, was carried away by the wind, and the storm was over.

It was from narratives such as the above that meteorologists began to turn their attention from electricity to the cyclonic action of the wind, as the efficient cause of the formation of hail. Thus Kämtz attributed such formation to the low temperature of the upper atmospheric strata in which the watery particles solidify, and Muncke to the meeting of cold and warm winds. Sir John Herschel²⁴ also suggested that the generation of hail seems always to depend on some very sudden introduction of an extremely cold current of air into the bosom of a quiescent, nearly saturated mass. So long ago as 1830 Professor Olmsted²⁵ realized this idea by means of the cyclone, in which a mass of air revolving round an axis more or less inclined to the earth is more or less highly rarefied at or about its vortex, and is thus in a condition to

²⁴ Scientific Essays.

²⁵ American Journal of Science, xvii. p. 1.

draw down cold air from above, or draw up warm moist air from below, in either case supplying some at least of the conditions for the generation of hail. The diminution of the temperature of the air with the altitude may be roughly stated as one degree F. for every 343 feet of ascent, and the point of perpetual congelation at and above the equator 14,000 feet; at 30° 12,000 feet; at 40° 10,000 feet; at 50° 8000 feet; at 60° 6000 feet; at 70° 4000 feet; at 80° 2000 feet, and after this the point rapidly approaches the earth.

Prof. Olmsted's theory has been admirably elaborated by the officers of the United States Coasts Survey, as will presently be noted. In the meantime a few cases may be cited to show how the idea gradually became developed into the present consistent theory. Thus Mr. J. C. Martin²⁶, of Pulborough in Sussex, writing in 1840, refers to masses of ice having fallen five, six, and seven inches in circumference, and goes on to state that there can be only one way by which such masses are suspended in the air long enough to grow to such a size, and that is by the assistance of a nubilar whirlwind or waterspout. He states that he once witnessed an appearance of this sort between a higher and a lower cloud, that had a strongly electric aspect before they had resolved themselves into *nimbus*. It was a bent massive column of dark vapour in rapid rotatory motion, passing from one cloud to the other, continuing for some minutes, and gradually disappearing. The hailstones are described as spheres flattened at the poles, the result of rotatory motion. In a hail-storm which devastated Dublin on April 18th, 1850²⁷, some observers state that they saw two strata of oppositely electrical clouds and discharges passing between them, and that the hailstones were as large as pigeons' eggs, and were formed of a nucleus of snow or sleet, surrounded by transparent ice; this was succeeded by an opaque white layer, followed by a second coating of ice, and, in some examples, five alternations were counted. The storm is described as a cyclone, but Mr. Piddington, in quoting it, prefers to call it a tornado. He also remarks on a common entry of the logs of ships, which have been involved in cyclones, and especially if near the centre, of "rain as cold as ice," "sea-water warm, rain bitterly cold;" also, "rain accompanied by sleet." In the account of the "Duke of York's" cyclone, the entry occurs twice—"Cold most intense during the hurricane." The enormous force with which hail is sometimes projected almost horizontally indicates a force very different from

²⁶ Quoted in Piddington's *Sailor's Horn-Book*, 1860.

²⁷ *Ibid.*

gravitation. In a storm described by Luke Howard²⁸, that occurred at Tottenham on the 19th April, 1809, at 5 p.m., the icy bullets, some of them a full inch in diameter, were discharged almost horizontally, and with such velocity that in many instances a clean round hole was left in the glass they pierced, and one large pane had two such perforations, distinctly formed, the glass being otherwise whole. The comparatively small width of the hail's track is also in favour of the cyclonic theory, although the length may be considerable. The great storm that began in the south of France early on the 13th of July, 1788, extended in a few hours over the whole kingdom, even as far as Holland. It proceeded in two parallel zones from S.W. to N.E.; one zone was 175 leagues in length, and the other 200; the breadth of the western zone was four leagues, and of the other only two. The zone between the two was five leagues wide, but no hail fell there, only heavy rain. There was also rain on the outer boundary of the two zones. A thick darkness accompanied the hail, and spread on both sides. The storm travelled at the rate of $16\frac{1}{2}$ leagues an hour in both zones. Upwards of a thousand parishes were ravaged by this storm.

Turning now to Mr. William Ferrel's theory²⁹ of the formation of hail in connexion with a tornado or cyclone, he calculates in an assumed example that the plane or stratum of zero temperature is 6428 metres above the base of the cloud, and in the absence of friction may be supposed to be brought down to the earth in the centre, where the gyrations are very rapid. Below this base aqueous vapour is condensed into cloud and rain, but above it into snow. The rain-drops below may also be carried up into the snow-region in the ascending currents, and if kept suspended there for a short time they may become frozen into small hail. They may then be kept suspended near the base of the snow-cloud, and increase in size by the rain, which is carried up into this region, coming into contact with them before it has had time to freeze. In this way compact homogeneous hailstones of ordinary size are formed. At the height of nearly 7000 metres the density of the air in comparison with that at the earth's surface is 0.42, and it is calculated in the assumed example that a velocity of 20 metres per second in the ascending current will sustain a hailstone one centimetre in diameter at that altitude. This is no unusual velocity for ascending currents in tornados. It is not necessary that the hailstones should remain long in

²⁸ Climate of London.

²⁹ United States Coasts Survey. Meteorological Remarks for the Use of the Coast Pilot, part ii. p. 85. Washington, 1880.

the freezing region, or even be stationary. They may be carried from the vortex out where the velocity of the ascending current is small, and dropping down some distance may then be carried in towards the vortex by the inflowing current on all sides, and up again rapidly into the freezing region.

The nucleus of large hailstones is generally composed of compact snow. A small ball of snow saturated with unfrozen rain, which is carried up into the snow-cloud, is formed in that region and freezes, and being of less specific gravity than compact hail is kept where it receives a thick coating from the rain carried up, as in the case of the small hail, and afterwards falls to the earth, either at some distance from the centre, where the ascending currents are weak, or near the vortex after the rapidity of the ascending currents has become sufficiently diminished. As there may be in the case of cloud-bursts a great accumulation of rain, and a sudden down-pouring of it, all in a short time, so in a hail-storm a great quantity of hail may be collected in the lower part of the cloud, brought in by inflowing currents on all sides towards the vortex, after the ascending currents have become too weak to carry it up and again throw it out above, and are still too strong to permit it to fall. But soon the interior of the tornado becomes so overloaded, and the energy of the whole system so much spent that the hail falls to the earth almost at once. Hence the large quantities of hail which sometimes fall in a short time.

When a hailstone is carried up in or near the vortex, and carried out above to where the ascending current is too feeble to sustain it in the air, it gradually drops down, and the inflowing current draws it in towards the vortex, where it is again carried up, and thus describes a sort of oval orbit. It may be thrown up very high into the snow-cloud region, or but little above its base. It may describe a number of such orbits or revolutions before it falls to the earth. While high up in the snow-cloud region it receives a coating of snow; and then, while descending very gently, where the strength of the currents is not quite sufficient to sustain it, and near the base of the snow region, where rain yet unfrozen is carried up, it receives a coating of solid ice, which may be continued for some time after it falls into the rain-cloud, since the hailstone still continues for some time below zero. After a short time the inflowing current below draws it again into the vortex, where it is again thrown up into the snow-region to receive a new coating of snow. It thus receives alternate coatings of snow and ice, and the number of each sort denotes the number of revolutions described before it falls to the

earth. When we consider the enormous amount of water which is rapidly carried up in a tornado, and that the lower part of the region of freezing must contain mostly rain not yet frozen, since the snow there formed is at once carried still higher, we can readily understand how the hailstone can receive a considerable coating of ice in a short time. While high up in the snow-cloud at its turning point, it of course remains some time nearly at the same altitude, and it is reasonable to suppose long enough to receive its coating of snow³⁰.

Hailstones vary greatly in shape as well as in weight. Some resemble a disk, or very oblate spheroid. If for any reason the hailstone becomes in the least flattened, the ascending current which keeps it suspended in the air also keeps its shortest diameter perpendicular to the current, and hence it increases most on the edges. Others are of a pyramidal form³¹.

Enormous masses of ice are reported to have fallen from the sky from time to time, but these seem to have consisted of a vast number of hailstones swept into hollows or cavities by the wind, and united by regelation. Nevertheless, some of the masses that are known on good authority to have fallen are sufficiently formidable. Mr. Darwin³² refers to cases in South America of hailstones sufficiently large to kill deer, and many cases are recorded of hailstones in India large enough to kill men and cattle³³. The hailstones chiefly occur in the driest months, February, March, and April; they are well known in the Delta of the Ganges down to the sea, in other places 1500 feet above the sea; in Ceylon the storms are formed by violent whirlwinds and eddies. Thus on May 12th, 1853, a storm occurred in the Himalayas, when the hailstones were very hard, compact and spherical,

³⁰ In a hailstorm at Northampton, Mass., June 20th, 1870, two hailstones fell weighing over half a pound. One is described in Silliman's Journal l. p. 403, consisting of thirteen layers, like the coats of an onion. It must have oscillated as many times between the rain-cloud and the snow-cloud region; that is, it performed six or seven revolutions with the lower part of its orbit in the rain-cloud, and the upper part in the snow-cloud.

³¹ On this subject see a paper by Professor Osborne Reynolds on Raindrops and Hailstones in 'Nature,' Dec. 21st, 1876.

³² Journal of a Naturalist.

³³ See a paper by Dr. George Buist, F.R.S., on Hailstorms in India, read before the British Association in 1835. The writer corrects the statements of Dr. Purdie Thompson and others that hailstorms are nearly unknown between the tropics.

many of them measuring $3\frac{1}{2}$ inches in diameter, or nearly a foot in circumference; 84 human beings and about 3000 oxen were killed. At Nainee Tal, a sanatorium in the Lower Himalayas, the noise of the approaching storm was as if thousands of bags of walnuts were being emptied in the air. The hail that first fell was of the size of pigeon's eggs, and at length became of the size of cricket balls. Dr. Buist describes the largest hailstones that fell in India as from 10 to 13 inches in circumference, and from 9 to 13 ounces in weight; the average maxima are from 8 to 10 inches in circumference, and 2 to 4 ounces in weight, but ordinary hail exceeds filberts in size.

In the accompanying Plate (IV.) I have collected some remarkable figures of hailstones, a number of which (1 to 8) fell during a storm which I witnessed at Leipzig on August 27th, 1860³⁴. I was proceeding by rail from Cassel to Leipzig; the day was hot, and the afternoon sultry, the thermometer marking 22° R. ($81\frac{1}{2}^{\circ}$ F.). About 4 p.m. copper-coloured clouds appeared in the west, the sky darkened, and about 6.30, when close to Leipzig, a black cloud, streaked with white bands, rose up like a pillar; there was a flash of lightning, and as soon as the thunder had ceased, a rattling noise was heard, which was succeeded by a shower of large hail. Just before this I had quitted the railway-station in a drosky, the flexible leathern covering of which was drawn down, and the windows on each side were up. The carriage had not quitted the station when a fiercely wailing wind twisted the leathern covering from its fastenings; it fell upon my head, when I felt a succession of rapid blows, and heard the crashing of the window-glass; the horse was rearing from fright, but the porters secured it, and brought the vehicle under cover. I picked up some of the hailstones, placed one on paper, and drew a pencil round it. It consisted of a nucleus of clear ice in the form of a flat spheroid, surrounded by semiopaque ice with lines radiating to near the circumference, as shown in fig. 1. Other stones were more irregular, as in fig. 2, where the opaque nucleus is surrounded by clear ice, this by opaque, while the outside layer is clear. Figs. 3, 4, 5 are from drawings made at the time, and inserted in the Leipzig illustrated papers. Figs. 6 and 7 represent a stone with ice crystals on a portion of the surface, while the other portion is smooth and rounded, as shown in the outline

³⁴ The details of this storm are abridged from a work of mine entitled "The Rain Cloud," published in 1876 by the Society for Promoting Christian Knowledge.

fig. 7. Fig. 8 seems to be a happy example of regelation. It broke through the studio window of the artist Georgy, who immediately made a sketch of it. It was of bright transparent ice, very hard and strong, with a cavity at the top large enough to admit the little finger. It was described as a perforated hailstone ; but the nucleus which occupied the cavity seems to have fallen out before the artist sketched it.

Some of the stones weighed 5 oz., and the damage to trees, crops, and fruit, glass windows and roofs was considerable ; curtains and blinds were torn into tatters ; the furniture of rooms, including pictures and mirrors, was also injured, and in the fields large numbers of hares and rabbits were killed. A curious example of the force of the icy bullets was shown in the destruction of the new cane-bottom of a chair. It would be supposed that so elastic a material would cause the hailstones to rebound. Zinc water-pipes were shot through, and in one case a pipe was flattened.

The extent of this hailstorm was about 25 miles in length by 5 miles in width, and the damage was very unequally distributed. The whirlwind character of the storm was noticed by many observers. The hail was preceded by rain-drops of large size, after which the rain and hail became mingled in one grey white fog, in which the leaves and twigs of trees, brought apparently from a distance, were seen whirling round. Other proofs of this whirling motion were shown in the unequal action of the storm in different parts of its comparatively narrow limits, and the various angles at which hail fell in different parts of Leipzig.

The storm of hail was over in about ten minutes, and the temperature fell from $81\frac{1}{2}^{\circ}$ F. to $45\frac{1}{2}^{\circ}$ F.

Figs. 9 and 10 represent in front and in section a beautiful example of the structure, so often referred to in this article, of alternate coats of opaque and transparent ice round a nucleus³⁵.

The four figures, 11 to 14, are from drawings made by my King's College colleague, Mr. H. Hatcher. The stones fell during a thunderstorm on the 22nd May, 1865. Fig. 11 shows layers of clear and opaque ice. Fig. 12 is apparently the end of a stone broken in the fall. Fig. 13 is a smaller stone of similar structure, and fig. 14 represents a stone showing a mammillated termination. Such was the general structure, but some of the smallest stones were nearly spherical, and entirely of clear glass-like ice. The largest stone observed

³⁵ These figures are from Buchan's *Meteorology*, 2nd edition, 1868.

was $\frac{5}{8}$ in. long, and $\frac{1}{2}$ in. in diameter. All the perfect stones (except those of clear ice) had a distinctly fibrous structure, and were more or less pear-shaped, unless broken in their fall. Some showed only two broad bands, others as many as five or six.

Figs. 15 and 16 represent two different forms of hailstone, which fell during a violent storm in Cambridgeshire, on August 9th, 1843. Some of the stones were so large that they stuck in a wine-glass³⁶.

Fig. 17 represents a hailstone that fell in Georgia on May 27th, 1869. It was picked up, together with others like it, and drawn at the time by Staatsrath Abich, and described by him in a letter to Chevalier W. von Haidinger³⁷. Similar stones also fell on June 9th at 6 p.m. They were $2\frac{1}{2}$ in. in diameter, spheroidal, of definite crystalline structure, overgrown along the plane of the major axis by a series of clear crystals, exhibiting various combinations belonging to the hexagonal system. The most abundant were combinations of the scalenohedron with rhombohedral faces, crystals $\frac{3}{4}$ in. in height, and corresponding thickness, prettily grouped, with combinations of the prism and obtuse rhombohedron. The terminal plane was also occasionally noticeable. Some which fell at the beginning of the storm were flat tabular crystalline masses $1\frac{1}{2}$ in. in diameter. The ring surrounding the nucleus had a milky appearance from the presence of small air-bubbles, as had the nucleus itself in most instances; many had also a clear nucleus. In melting down, some of the stones took the shape of a regular hexagon. The milky ring round the central point was a sort of fibrous web, composed of the finest air-cavities, traversed by thread-like pores. Some of the air-bubbles were pear-shaped or worm-like, running from centre to circumference. The crystals were attached parasitically to the edge of the stone, or else inserted in a kind of socket, as was noticed when the stones thawed down.

Highgate, N., July 1889.

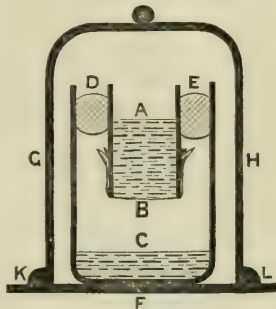
³⁶ Observations in Meteorology, by the Rev. R. Jenyns, M.A., F.L.S.

³⁷ Journal of the Austrian Meteorological Society, iv. p. 417, translated into the Annual Report of the Board of Regents of the Smithsonian Institution for 1869. Professor Henry was kind enough to send me a copy of this work.

XXVII. *On Endless Availability; and on a Restriction to the Application of Carnot's Principle.* By CHARLES V. BURTON, D.Sc.*

IN the following pages some experiments are described which appear to be in obvious disagreement with the Second Law of Thermodynamics. The first apparatus used is shown in fig. 1. A piece of glass tube, A, is closed below by a dialysing membrane, B, tied over it in the usual manner. It contains a solution to be dialysed, and is supported within a beaker, C, by wedges of cork, D, E. The beaker, C, stands on a glass plate, F, the whole being covered by a bell-jar, G H, which is luted with wax round the edge, K L, so that evaporation from the solution in the dialyser is prevented as far as possible. The beaker, C, is at first empty; and when a certain portion of the solution in A has passed into it through the membrane B, the process is stopped by taking the apparatus to pieces and mixing the solutions. A thermometer measures the rise or fall of temperature which ensues.

Fig. 1.



Experiment I.—A saturated solution of normal sodium sulphate ($\text{Na}_2\text{SO}_4, 10\text{H}_2\text{O}$) was placed in the dialyser, together with a crystal of the salt. Contrary to anticipation, it was found after some days that the crystal had entirely disappeared and about $\frac{3}{4}$ of the solution had passed through the dialyser. When the experiment had lasted 14 days, about $\frac{5}{6}$ of the solution had passed through. The solutions in A and C were then mixed, and the temperature rose $1^{\circ}2$ (Centigrade). By next day some crystals had been deposited from the mixed solutions; and had this crystallization taken place under adiabatic conditions, there must have been a further elevation of temperature. The following are the details of the experiment:—

1888. Oct. 26; 5.10 P.M. Solution of sodium sulphate saturated at 15° , together with a crystal of the salt, placed in dialyser and completely protected from evaporation.

Nov. 9; 4.5 P.M. About $\frac{5}{6}$ of the solution has passed through; no crystal remains.

* Communicated by the Author.

Temp. of thermometer, which has lain beside the apparatus for 14 days . . .	$= 10^{\circ} 2$
Temp. of solutions in A and in C . . .	$= 14^{\circ} 2$
Temp. of solutions after mixing . . .	$= 15^{\circ} 4$
Rise of temperature	$= 1^{\circ} 2$

Nov. 10. Crystals, apparently about equal to the original crystal in A, have been deposited.

Thus we can perform a complete cycle of changes. Starting with saturated solution and crystals of sodium sulphate in the dialyser, at the temperature of surrounding objects, an isothermal change first takes place. Next the separated portions of the solutions are mixed, and may be maintained under adiabatic conditions till all possible crystallization has taken place, evaporation being of course excluded. The result is a considerable rise of temperature, with corresponding gain of motivity; and finally, when the liquid has been cooled down (with further deposition of crystal) to the initial temperature, we have come back to precisely the conditions with which we started—a saturated solution and crystals of sodium sulphate, at the temperature of surrounding objects.

Now consider what is the action of the dialysing membrane. According to the view of Arrhenius, dissociation and recombination are continually occurring amongst the molecules of the solution, each dissociation being accompanied by an absorption of heat, and each recombination by an equal evolution of heat. By filtering such a solution through a membrane, the equilibrium of these processes is disturbed. The various chemical constituents will pass through at different rates, thus giving rise to chemical separation and (isothermal) absorption of heat*. The membrane then plays the part of a sieve: it does not really cause dissociation; it only effects a selective distribution of molecules already dissociated. If reliance could be placed in the constancy of a dialysing membrane, and if the composition, temperature, and level of the liquid above the membrane were also maintained constant, an analysis of the portion which passes through might furnish some conclusions as to the amount of dissociation in the solution. It would be interesting to compare such results with the data afforded by measurements of electrolytic conductivity.

Experiment II.—The acid sodium sulphate being more soluble than the normal sulphate, the more acid liquid which

* The solution remaining in the dialyser is alkaline, and therefore non-saturated; hence the crystal dissolves.

passes through the dialyser will be able to dissolve some more sodium-sulphate crystals. The apparatus (fig. 1) was accordingly arranged with excess of crystals in the beaker C, as well as in the dialyser A. The following are the details of the experiment :—

1888. Nov. 20 ; 5 P.M. Dialyser set up with 3 layers of parchment-paper and excess of crystals ($\text{Na}_2\text{SO}_4, 10\text{H}_2\text{O}$) in both A and C. Saturated sodium-sulphate solution in A. Apparatus completely protected from evaporation.

Dec. 5 ; 4 P.M. Temp. of solutions (before mixing) = $14^{\circ}\cdot 4$.

On pouring both solutions into a stoppered bottle which had lain some days beside the apparatus, the temp. rose to $15^{\circ}\cdot 2$.

Rise of temperature = $0^{\circ}\cdot 8$.

Bottle now stoppered (one mgrm. sodium-sulphate crystals added to promote further crystallization).

Dec. 10. Crystals have formed in the bottle.

Temp. of solution . . .	= $14^{\circ}\cdot 2$
Mass of solution . . .	= $26\cdot 156$ grams
Mass of crystals . . .	= $2\cdot 446$ „

Now I find the latent heat of solution of crystallized sodium sulphate to be $64\cdot 6$, and the specific heat of a saturated solution at 15° about $\cdot 97$; so that if the mixed solutions had been kept under adiabatic conditions while the crystals were forming, there would have been a total rise of temperature of more than 3 degrees (due allowance being made for the increased solubility at higher temperatures).

The mechanical availability would then be

$$J \int_{273+14\cdot 4}^{273+17\cdot 4} \frac{k \cdot M \cdot (\theta - 273 + 14\cdot 4)}{\theta} d\theta,$$

where M is the mass of the substance, and k its specific heat at the temperature θ . Putting

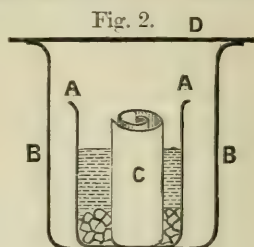
$$k = \text{constant} = \cdot 97,$$

the above expression becomes

$$\cdot 97 \times JM \left\{ (290\cdot 4 - 287\cdot 4) - 287\cdot 4 \log_e \frac{290\cdot 4}{287\cdot 4} \right\} \\ = \text{about } 500,000 \text{ M ergs.}$$

That is, after descending under gravity through an average height of about 2 cm., the solution has increased the motivity of the system by an amount sufficient to raise itself vertically through more than 5 metres.

Experiment III.—The apparatus was arranged as follows:—A beaker, A (fig. 2), contained saturated solution and crystals of sodium sulphate, in which some parchment-paper, C, was partly immersed, so as to increase the surface for evaporation; A was placed within a larger beaker, B, which was closed by a glass plate, D, luted on with soft wax so as to be air-tight. The apparatus was left in a dark corner, and after a day or two, dew was seen to have collected on the sides of the outer beaker, B. After six weeks, about half a gram of water had collected in the beaker.



Now, undoubtedly the apparatus underwent changes of temperature; but these would be essentially very slow, so that the difference of temperature between one part of the apparatus and another would be extremely small, and this must have been especially the case between the surface of the solution and the adjacent walls of the beaker A, where dew had also formed and gradually increased in amount.

From the *continued* distillation it seemed probable that the vapour-pressure over thoroughly saturated sodium-sulphate solution is slightly greater than over pure water. It is evident that the saturated solution has the smaller latent heat of vaporization, since evaporation is then always accompanied by crystallization. Hence a water-molecule expends less energy in attaining the gaseous state than would be the case at the surface of pure water.

Here, again, we can perform a complete cycle of operations. The water which has distilled may be collected, and the deposited crystal dissolved in it. A fall of temperature will result, which will render some of the heat of surrounding objects available for mechanical work. When the resulting solution has been raised to its previous temperature, it will just be saturated; and on pouring it back into the beaker A, the initial conditions are exactly restored.

A direct measurement was also made of the vapour-pressure over saturated sodium-sulphate solution (without parchment-paper). A flask containing a thick paste of sodium-sulphate solution and powdered crystals was immersed in a bath of water, and was connected by a rubber tube to a Sprengel pump and a barometer-tube. The vapour-pressure at $12^{\circ}72$ was found to be 10.7 mm., which differs from the vapour-pressure over pure water at the same temperature by only

about -0.2 mm.; while Wüllner* found for a (non-saturated) 15-per-cent. solution of sodium sulphate at $26^{\circ}.3$, a vapour-pressure 1.2 mm. less than that of pure water. Hence it seems that, just at the point where the solution becomes fully saturated, there is a discontinuity in its vapour-pressure.

According to Regnault's classical researches, a vapour-pressure of 10.7 mm. over pure water corresponds to a temperature of $12^{\circ}.40$ (instead of $12^{\circ}.72$). The influence of the parchment-paper wick remains to be determined. But even should it produce no increase in the vapour-pressure, there would still be, on the whole, a considerable *gain* of motivity in experiment III.

In all these experiments the working substance becomes separated into two portions, which are not identical in chemical composition. I would therefore suggest this restriction, *that we cannot as yet assume with certainty the truth of Carnot's Principle when chemical separation occurs between two finite portions of the working substance.* Further research seems necessary before we can say whether or not this is the only exception to the truth of Carnot's Principle.

XXVIII. *On Achromatic Interference-Bands.* By LORD RAYLEIGH, *Sec. R.S., Professor of Natural Philosophy in the Royal Institution.*

[Concluded from p. 91.]

Airy's Theory of the White Centre.

IF a system of interference-bands be examined through a prism, the central white band undergoes an abnormal displacement, which has been supposed to be inconsistent with theory. The explanation has been shown by Airy† to depend upon the peculiar manner in which the white band is in general formed. Thus, "Any one of the kinds of homogeneous light composing the incident heterogeneous light will produce a series of bright and dark bars, unlimited in number so far as the mixture of light from the two pencils extends, and undistinguishable in quality. The consideration, therefore, of homogeneous light will never enable us to determine which is the point that the eye immediately turns to as the centre of the fringes. What, then, is the physical circumstance that determines the centre of the fringes?"

"The answer is very easy. For different colours the bars

* Pogg. *Ann.* ciii. p. 543.

† Airy, "Remarks on Mr. Potter's Experiment on Interference," *Phil. Mag.* ii. p. 161 (1833).

have different breadths. If, then, the bars of all colours coincide at one part of the mixture of light, they will not coincide at any other part; but at equal distances on both sides from that place of coincidence they will be equally far from a state of coincidence. If, then, we can find where the bars of all colours coincide, that point is the centre of the fringes.

“It appears, then, that the centre of the fringes is *not* necessarily the point where the two pencils of light have described equal paths, but is determined by considerations of a perfectly different kind. . . . The distinction is important in this and other experiments.”

The effect in question depends upon the dispersive power of the prism. If v be the linear shifting due to the prism of the originally central band, v must be regarded as a function of λ . Measured from the original centre, the position of the n th bar is now

$$v + n\lambda D/b.$$

The coincidence of the various bright bands occurs when this quantity is as independent as possible of λ ; that is, when n is the nearest integer to

$$n = -\frac{b}{D} \frac{dv}{d\lambda}; \quad (18)$$

or, as Airy expresses it, in terms of the width of a band (Λ),

$$n = -dv/d\Lambda. \quad (19)$$

The apparent displacement of the white band is thus not v simply, but

$$v - \Lambda dv/d\Lambda. \quad (20)$$

The signs of dv and $d\Lambda$ being opposite, the abnormal displacement is in addition to the normal effect of the prism. But, since $dv/d\Lambda$, or $dv/d\lambda$, is not constant, the achromatism of the white band is less perfect than when no prism is used.

If a grating were substituted for a prism, v would vary as Λ , and the displacement (20) would vanish.

More recently the matter has engaged the attention of Cornu*, who thus formulates the general principle:—“*Dans un système de franges d'interférence produites à l'aide d'une lumière hétérogène ayant un spectre continu, il existe toujours une frange achromatique qui joue le rôle de frange centrale et qui se trouve au point de champ où les radiations les plus intenses présentent une différence de phase maximum ou minimum.*”

In Fresnel's experiment, if the retardation of phase due to an interposed plate, or to any other cause, be $F(\lambda)$, the whole

* *Journ. d. Physique*, i. p. 293 (1882).

and the retardation in *phase* is $2t \cos \alpha' / \lambda$, λ being as usual the wave-length in air.

"The first thing to be noticed is that, when α approaches the critical angle, $\cos \alpha'$ becomes as small as we please, and that, consequently, the retardation corresponding to a given thickness is very much less than at perpendicular incidence. Hence the glass surfaces need not be so close as usual.

"A second feature is the increased brilliancy of the light. But the peculiarity which most demands attention is the lessened influence of a variation in λ upon the phase retardation. A diminution of λ of itself increases the retardation of phase, but since waves of shorter wave-length are more refrangible, this effect may be more or less perfectly compensated by the greater obliquity, and consequent diminution in the value of $\cos \alpha'$. We will investigate the conditions under which the retardation of phase is stationary in spite of a variation of λ .

"In order that $\lambda^{-1} \cos \alpha'$ may be stationary, we must have

$$\lambda \sin \alpha' d\alpha' + \cos \alpha' d\lambda = 0,$$

where (α being constant)

$$\cos \alpha' d\alpha' = \sin \alpha d\mu.$$

Thus

$$\cot^2 \alpha' = - \frac{\lambda d\mu}{\mu d\lambda}, \quad . \quad . \quad . \quad . \quad . \quad (23)$$

giving α' when the relation between μ and λ is known.

"According to Cauchy's formula, which represents the facts very well throughout most of the visible spectrum,

$$\mu = A + B\lambda^{-2}, \quad . \quad . \quad . \quad . \quad . \quad (24)$$

so that

$$\cot^2 \alpha' = \frac{2B}{\lambda^2 \mu} = \frac{2(\mu - A)}{\mu}. \quad . \quad . \quad . \quad . \quad (25)$$

If we take, as for Chance's 'extra-dense flint,'

$$B = .984 \times 10^{-10},$$

and, as for the soda-lines,

$$\mu = 1.65, \quad \lambda = 5.89 \times 10^{-5},$$

we get

$$\alpha' = 79^\circ 30'.$$

At this angle of refraction, and with this kind of glass, the retardation of phase is accordingly nearly independent of wave-length, and therefore the bands formed, as the thickness varies, are approximately achromatic."

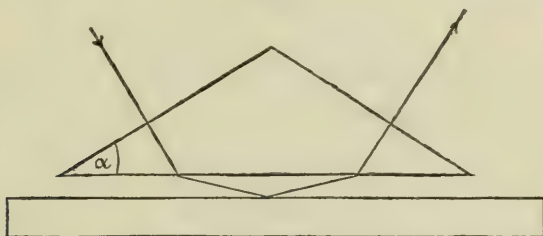
Perfect achromatism would be possible only under a law of dispersion*

$$\mu = A\lambda^{-c^2}, \quad . \quad . \quad . \quad . \quad . \quad (26)$$

where A and c^2 are constants, of which the latter denotes the value of $\cot^2 \alpha'$.

The above investigation, as given in the *Enc. Brit.*, was intended to apply to Talbot's manner of experimenting, and it affords a satisfactory explanation of the formation of achromatic bands. In order to realize the nearly grazing incidence, the plate of air must be bounded on one side by a prism (fig. 1). Upon this fall nearly parallel rays from a

Fig. 1.



“radiant point of solar light,” obtained with the aid of a lens of short focus. The bands may be observed upon a piece of ground glass held behind the prism in the reflected light, or they may be received directly upon an eyepiece.

These bands undoubtedly correspond to varying thicknesses of the plate of air, just as do the ordinary Newton's rings formed at nearly perpendicular incidence. For theoretical purposes we have the simplest conditions, if we suppose the thickness uniform, and that all the rays incident upon the plate are strictly parallel. Under these suppositions the field is uniform, the brightness for any kind of light depending upon the precise thickness in operation. If the thickness be imagined to increase gradually from zero, we are presented with a certain sequence of colours. When, however, the relation (23) is satisfied, the formation of colour is postponed, and the series commences with a number of alternations of black and white. In actual experiment it would be difficult to realize these conditions. If the surfaces bounding the plate are inclined to one another, the various parts of the field correspond to different thicknesses; and, at any rate if the inclination be small, there is presented at one view a series of colours, constituting bands, the same as could only

* A mistake is here corrected.

be seen in succession were the parallelism maintained rigorously.

The achromatism secured by (23) not being absolute, it is of interest to inquire what number of bands are to be expected. The relative retardation of phase, with which we have to deal, is $2t \cos \alpha' / \lambda$, or

$$\frac{2t \sqrt{(1 - \mu^2 \sin^2 \alpha)}}{\lambda} \dots \dots \dots (27)$$

If this be stationary for extra-dense glass and for the line D, we have, as already mentioned, $\alpha' = 79^\circ 30'$, and corresponding thereto $\alpha = 36^\circ 34'$. Taking this as a prescribed value of α , we may compare the values of (27) for the lines C, D, E, using the data given by Hopkinson*, viz.:—

$$C, \quad \mu = 1.644866, \quad \lambda = .65618 \times 10^{-4}$$

$$D, \quad \mu = 1.650388, \quad \lambda = .58890 \times 10^{-4}$$

$$E, \quad \mu = 1.657653, \quad \lambda = .52690 \times 10^{-4}.$$

We find

$$\text{for C} \quad (27) = 3036.9 \times 2t,$$

$$D \quad (27) = 3094.5 \times 2t,$$

$$E \quad (27) = 2984.3 \times 2t.$$

These retardations are reckoned in periods. If we suppose that the retardation for the C-system is just half a period less than for the D-system, we have

$$57.6 \times 2t = \frac{1}{2};$$

so that $t = \frac{1}{2 \cdot 576}$ centim. Thus about 27 periods of the D-bands correspond to $26\frac{1}{2}$ of the C-bands.

If the range of refrangibility contemplated be small, the calculation may conveniently be conducted algebraically. According to Cauchy's law we may replace (27) by

$$\frac{2t \sqrt{(1 - \mu^2 \sin^2 \alpha)} (\mu - A)}{\sqrt{B}} \dots \dots \dots (28)$$

Setting $\mu = \mu_0 + \delta\mu$, we have approximately

$$\begin{aligned} (1 - \mu^2 \sin^2 \alpha) (\mu - A) &= (1 - \mu_0^2 \sin^2 \alpha) (\mu_0 - A) \\ &+ \delta\mu \{1 - \mu_0^2 \sin^2 \alpha - 2\mu_0 \sin^2 \alpha\} (\mu_0 - A) \} \\ &- (\delta\mu)^2 \{3\mu_0 - A\} \sin^2 \alpha + \dots \end{aligned}$$

If α be so chosen that the value of (28) is stationary for μ_0 , the term of the first order in $\delta\mu$ vanishes, and we obtain

* Proc. Roy. Soc., June 1877.

finally as the approximate value of (28)

$$\frac{2t \sin \alpha (\mu_0 - A) \sqrt{2\mu_0}}{\sqrt{B}} \cdot \left\{ 1 - \frac{(\beta\mu_0 - A)(\delta\mu)^2}{4\mu_0(\mu_0 - A)^2} \right\}. \quad (29)$$

If now the circumstances be such that n periods of the μ_0 system correspond to $n - \frac{1}{2}$ of the μ system,

$$\frac{1}{n} = \frac{(\beta\mu_0 - A)(\delta\mu)^2}{2\mu_0(\mu_0 - A)^2}, \quad \dots \quad (30)$$

in which the ratio of $(\beta\mu_0 - A)$ to $2\mu_0$ does not differ much from unity. In the application to extra-dense flint the simplified formula

$$n = (\mu_0 - A)^2 / (\mu - \mu_0)^2 \quad \dots \quad (31)$$

gives very nearly the same result as that previously found. The number of bands which approximately coincide is inversely as the square of the range of refrangibility included.

It must not be overlooked that the preceding investigation, though satisfactory so far as it goes, is somewhat special on account of the assumption that the angle of incidence (α) upon the plate of air is the same for the various colours. If the rays are parallel before they fall upon the prism, they cannot remain parallel unless the incidence upon the first surface be perpendicular. There is no reason why this should not be the case; but it is tantamount to a restriction upon the angle of the prism, since α is determined by the achromatic condition. If the angle of the prism be other than α , the required condition will be influenced by the separation of the colours upon first entering the glass. Although the general character of the phenomenon is not changed, it may be well to give the calculation applicable to all angles of prism, as was first done by M. Mascart.

Denoting, as before, by α, α' the angles of incidence and refraction upon the plate of air, let β', β be the angles of incidence and refraction at the first surface of the prism (fig. 2), whose angle is A . Then, if Δ , equal to $n\lambda$, be the retardation,

$$\Delta = n\lambda = 2t \cos \alpha', \quad \dots \quad (32)$$

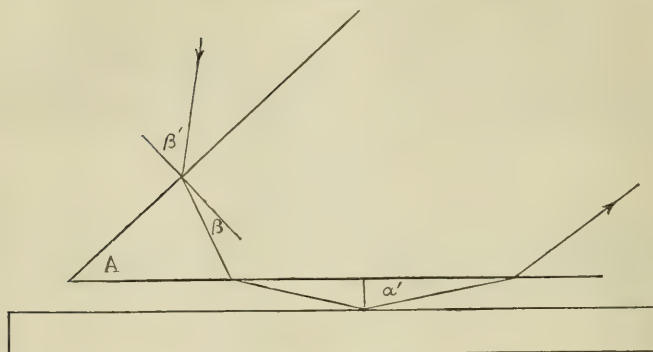
as before; while the relations among the angular quantities are:—

$$\sin \alpha' = \mu \sin \alpha, \quad \dots \quad (33)$$

$$\alpha + \beta = A, \quad \dots \quad (34)$$

$$\sin \beta' = \mu \sin \beta. \quad \dots \quad (35)$$

Fig. 2.



We have now to inquire under what conditions Δ/λ , or n , will be stationary, in spite of a variation of λ , if β' be constant. Thus

$$\lambda \sin \alpha' d\alpha' + \cos \alpha' d\lambda = 0,$$

while

$$\cos \alpha' d\alpha' = d\mu \sin \alpha + \mu \cos \alpha d\alpha,$$

$$d\alpha + d\beta = 0,$$

$$0 = d\mu \sin \beta + \mu \cos \beta d\beta.$$

Accordingly,

$$\begin{aligned} -\frac{\cot \alpha' d\lambda}{\lambda} \cos \alpha' &= d\mu \sin \alpha + \mu \cos \alpha d\alpha \\ &= d\mu \sin \alpha - \mu \cos \alpha d\beta \\ &= d\mu \sin \alpha + \cos \alpha \tan \beta d\mu \\ &= \sin A d\mu / \cos \beta; \end{aligned}$$

so that

$$\cot^2 \alpha' = -\frac{\lambda d\mu}{\mu d\lambda} \frac{\sin A}{\sin \alpha \cos \beta} \quad \dots \quad (36)$$

is the condition that n should be stationary. In the more particular case considered above, $\beta' = 0$, $\beta = 0$, $\alpha = A$.

These bands, which I should have been inclined to designate after Talbot, were it not that his name is already connected with another very remarkable system of bands, are readily observed. For the "radiant point of solar light" we may substitute, if more convenient, that of the electric arc. A small hole in a piece of metal held close to the arc allows sufficient light to pass if the bands are observed without the intervention of a diffusing-screen. At a distance of say 20 feet the nearly parallel rays fall upon the prism* and

* A right-angled isosceles prism ($A = 45^\circ$) answers very well. The plate should be blackened at the hind surface; or it may be replaced by a second prism.

plate, which should be mounted in such a fashion that the pressure may be varied, and that the whole may be readily turned in azimuth. The coloured bands are best seen when the surfaces are nearly parallel and pretty close. It is best to commence observations under these conditions. When the achromatic azimuth has been found, the interval may be increased. If it is desired to see a large number of bands, a strip of paper may be interposed between the surfaces along one edge, so as to form a plate of graduated thickness. Talbot speaks of from 100 to 200 achromatic bands; but I do not think any such large number can be even approximately achromatic. The composition of the light may be studied with the aid of a pocket spectroscope, and the appearances correspond with what has been already described under the head of interference-bands formed from a prismatic spectrum in place of the usual line of undecomposed light. As has been already remarked, the colours of fine bands are difficult to appreciate; and indistinctness is liable to be attributed to other causes when really due to insufficient achromatism.

The use of a wedge-shaped layer of air is convenient in order to obtain a simultaneous view of a large number of bands; but it must not be overlooked that it involves some departure from theoretical simplicity. The proper development of the light due to any thickness requires repeated reflexions to and fro within the layer, and at a high degree of obliquity this process occupies a considerable width. If the band-interval be too small, complications necessarily ensue, which are probably connected with the fact that the appearance of the bands changes somewhat according to the distance from the reflecting combination at which they are observed.

Herschel's Bands.

In the system of bands above discussed, substantially identical (I believe) with those observed by Talbot, all the rays of a given colour are refracted under constant angles, the variable element being the thickness of the plate of air. A system in many respects quite distinct was described by W. Herschel, and has recently been discussed by M. Mascart*. In this case the combination of prism and plate remains as before, but the thickness of the film of air is considered to be constant, the alternations constituting the bands being dependent upon the varying angles at which the light (even though of given colour) is refracted. In order to see these bands all that is necessary is to view a source of light presenting a large angle, such as the sky, by reflexion in the layer of air. They are

* *Loc. cit.*; also *Traité d'Optique*, tom. i. Paris, 1889.

formed a little beyond the limit of total reflexion. They are broad and richly coloured if the layer of air be thin, but as the thickness increases they become finer, and the colour is less evident.

The theoretical condition of constant thickness is better satisfied if (after Mascart) we place the layer of air in the focus of a small radiant point (*e. g.* the electric arc) as formed by an achromatic lens of wide angle. In this case the area concerned may be made so small that the thickness in operation can scarcely vary, and the ideal Herschel's bands are seen depicted upon a screen held in the path of the reflected light. It will of course be understood that bands may be observed of an intermediate character in the formation of which both thickness and incidence vary. Herschel's observations relate to one particular case—that of constant thickness; Talbot's to the other especially simple case of constant angle of incidence.

From our present point of view there is, however, one very important distinction between the two systems of bands. The one system is achromatic, and the other is not. In order to understand this, it is necessary to follow in greater detail the theory of Herschel's bands.

We will commence by supposing that the light is homogeneous (λ constant), and inquire into the law of formation of the bands, t being given. The same equations, (32) &c., apply as before, and also fig. 2, if we suppose the course of the rays reversed, so that the direction of the *emergent* ray is determined by β' . The question to be investigated is the relation of β' to n , and to the other data of the experiment.

The band of zero order ($n=0$) occurs when $\alpha'=90^\circ$, that is at the limit of total reflexion. The corresponding values of α , β , and β' may be determined in succession from (33), (34), (35). The value of α' for the n th band is given immediately by (32). For the width of the band, corresponding to the change of n into $n+1$, we have

$$\lambda = -2t \sin \alpha' d\alpha',$$

and from the other equations,

$$\cos \alpha' d\alpha' = \mu \cos \alpha d\alpha,$$

$$d\alpha + d\beta = 0,$$

$$\cos \beta' d\beta' = \mu \cos \beta d\beta;$$

so that the apparent width of the n th band is given by

$$d\beta' = \frac{n\lambda^2}{4t^2} \frac{\cos \beta}{\cos \beta' \cos \alpha \sin \alpha'} \dots \dots (37)$$

In the neighbourhood of the limit of total reflexion $\sin \alpha'$

is nearly equal to unity, and the factors $\cos \beta$, $\cos \beta'$, $\cos \alpha$ vary but slowly with the order of the band and also with the wave-length. Hence the width of the n th band is approximately proportional to the order, to the square of the wave-length, and to the inverse square of the thickness.

This series of bands, commencing at the limit of total reflexion, and gradually increasing in width, are easily observed with Herschel's apparatus by the aid of a soda-flame. In order to increase the field of view, the flame may be focussed upon the layer of air by a wide-angled lens. The eye should be adjusted for distant objects, and the thickness of the layer should be as uniform as possible. For the latter purpose the glass surfaces may be pressed against two strips of rather thin paper, interposed along two opposite edges.

We have now to consider what happens when the source of light is white. According to Airy's principle the centre of the system is to be found where there is coincidence of bands of order n , in spite of a variation of λ . This is precisely the question already dealt with in connexion with the other system of bands, and the answer is embodied in (36). About the achromatic centre thus determined will the visible bands be grouped.

And now the question arises, Are these bands achromatic? Certainly not. M. Mascart, to whom is due equation (37), appears to me to misinterpret it when he concludes that the bands are approximately achromatic*. At the central band n is the same for the various colours. Consequently the widths of the various systems *at this place* are approximately proportional to λ^2 . It will be seen that, so far from the system being achromatic, it is much less so than the ordinary system of interference-bands, or of Newton's rings, in which the width is proportional to the *first* power of λ . And this theoretical conclusion appears to me to be in harmony with observation.

At first sight it may appear strange that an achromatic centre should be possible with bands proportional to λ^2 . The explanation depends upon the fact that the limit of total reflexion, where the bands commence, is itself a function of λ .

The apparent width of the visible bands depends upon t , but is not, as might erroneously be supposed, proportional to t^{-2} . For this purpose n in (37) must be regarded as a function of t . In fact, by (32), if α' be given, n varies as t/λ ; so that, in estimating the influence of t , other circumstances remaining

* *Traité d'Optique*, t. i. p. 451. "On s'explique ainsi que la largeur apparente des franges voisines de la frange achromatique soit à peu près indépendante de la longueur d'onde dans une ouverture angulaire notable et qu'on en distingue un grand nombre."

unaltered, the width is proportional to t^{-1} . Hence, as the interval between the surfaces increases, the bands become finer, but the centre does not shift, nor is there any change in their number as limited by the advent of chromatic confusion.

Effect of a Prism upon Newton's Rings.

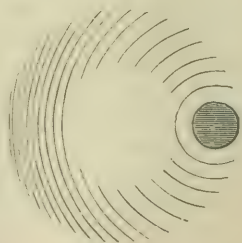
If Newton's rings are examined through a prism, some very remarkable phenomena are exhibited, described in his 24th observation*.

"When the two object-glasses were laid upon one another, so as to make the rings of the colours appear, though with my naked eye I could not discern above 8 or 9 of these rings, yet by viewing them through a prism I have seen a far greater multitude, insomuch that I could number more than 40, besides many others which were so very small and close together that I could not keep my eye steady on them severally so as to number them, but by their extent I have sometimes estimated them to be more than a hundred. And I believe the experiment may be improved to the discovery of far greater numbers; for they seem to be really unlimited, though visible only so far as they can be separated by the refraction, as I shall hereafter explain.

"But it was but one side of these rings—namely, that towards which the refraction was made—which by that refraction was rendered distinct; and the other side became more confused than when viewed by the naked eye, insomuch that there I could not discern above 1 or 2, and sometimes none of those rings, of which I could discern 8 or 9 with my naked eye. And their segments or arcs, which on the other side appeared so numerous, for the most part exceeded not the third part of a circle. If the refraction was very great, or the prism very distant from the object-glasses, the middle part of those arcs became also confused, so as to disappear and constitute an even whiteness, while on either side their ends, as also the whole arcs furthest from the centre, became distincter than before, appearing in the form as you see them designed in the fifth figure [fig. 3]."

"The arcs, where they seemed distinctest, were only black and white successively, without any other colours intermixed. But in other places there appeared colours, whose order was

Fig 3.



* Opticks. See also Place, *Pogg. Ann.* cxiv. p. 504 (1861).

inverted by the refraction in such manner that if I first held the prism very near the object-glasses, and then gradually removed it further off towards my eye, the colours of the 2nd, 3rd, 4th, and following rings shrunk towards the white that emerged between them, until they wholly vanished into it at the middle of the arc, and afterwards emerged again in a contrary order. But at the ends of the arcs they retained their order unchanged."

"I have sometimes so laid one object-glass upon the other, that to the naked eye they have all over seemed uniformly white, without the least appearance of any of the coloured rings; and yet, by viewing them through a prism, great multitudes of these rings have discovered themselves. And in like manner, plates of Muscovy glass, and bubbles of glass blown at a lamp-furnace, which were not so thin as to exhibit any colours to the naked eye, have through the prism exhibited a great variety of them ranged irregularly up and down in the form of waves. And so bubbles of water, before they began to exhibit their colours to the naked eye of a bystander, have appeared through a prism, girded about with many parallel and horizontal rings: to produce which effect it was necessary to hold the prism parallel, or very nearly parallel, to the horizon, and to dispose it so that the rays might be refracted upwards."

Newton was evidently much struck with these "so odd circumstances," and he explains the occurrence of the rings at unusual thicknesses as due to the dispersing power of the prism. The blue system being more refracted than the red, it is possible, under certain conditions, that the n th blue ring may be so much displaced relatively to the corresponding red ring as *at one part of the circumference* to compensate for the different diameters. White and black stripes may thus be formed in a situation where, without the prism, the mixture of colours would be complete, so far as could be judged by the eye.

The simplest case that can be considered is when the "thin plate" is bounded by plane surfaces inclined to one another at a small angle. Without the prism, the various systems coincide at the bar of zero order. The width of the bands is constant for each system, and in passing from one system to another is proportional to λ . Regarded through a prism of small angle whose refracting edge is parallel to the intersection of the bounding surfaces of the plate, the various systems no longer coincide for zero order; but by drawing back the prism, it will always be possible so to adjust the effective dispersing power as to bring the n th bars to coincidence for any two assigned colours, and therefore approximately for the

entire spectrum. The formation of the achromatic band, or rather central black bar, depends indeed upon precisely the same principles as the fictitious shifting of the centre of a system of Fresnel's bands when viewed through a prism.

In this example the formation of visible rings at unusual thicknesses is easily understood ; but it gives no explanation of the increased numbers observed by Newton. The width of the bands for any colour is proportional to λ , as well after the displacement by the prism as before. The manner of overlapping of two systems whose n th bars have been brought to coincidence is unaltered ; so that the succession of colours in white light, and the number of perceptible bands, is much as usual.

In order that there may be an *achromatic system* of bands, it is necessary that the width of the bands near the centre be the same for the various colours. As we have seen, this condition cannot be satisfied when the plate is a true wedge ; for then the width for each colour is proportional to λ . If, however, the surfaces bounding the plate be *curved*, the width for each colour varies at different parts of the plate, and it is possible that the blue bands from one part, when seen through the prism, may fit the red bands from another part of the plate. Of course, when no prism is used, the sequence of colours is the same whether the boundaries of the plate be straight or curved.

For simplicity we will first suppose that the surfaces are still cylindrical, so that the thickness is a function of but one coordinate x , measured in the direction of refraction. If we choose the point of nearest approach as the origin of x , the thickness may be taken to be

$$t = a + bx^2, \quad . \quad . \quad . \quad . \quad . \quad . \quad (38)$$

a being thus the least distance between the surfaces. The black of the n th order for wave-length λ occurs when

$$\frac{1}{2}n\lambda = a + bx^2; \quad . \quad . \quad . \quad . \quad . \quad . \quad (39)$$

so that the width (δx) of the band at this place (x) is given by

$$\frac{1}{2}\lambda = 2bx \delta x,$$

or

$$\delta x = \lambda/4bx. \quad . \quad . \quad . \quad . \quad . \quad . \quad (40)$$

Substituting for x from (38), we obtain, as the width of the band of n th order for any colour,

$$\delta x = \frac{\lambda}{4\sqrt{b} \cdot \sqrt{(\frac{1}{2}n\lambda - a)}}. \quad . \quad . \quad . \quad . \quad . \quad (41)$$

It will be seen that, while at a *given part of the plate* the

width is proportional to λ , the width for the n th order is a different function dependent upon a . It is with the latter that we are concerned when, by means of the prism, the n th bars have been brought to coincidence.

If the glasses be in contact, as is usually supposed in the theory of Newton's rings, $a=0$; and therefore, by (41), $\delta x \propto \lambda^{\frac{1}{2}}$, or the width of the band of the n th order varies as the square root of the wave-length, instead of as the first power. Even in this case the overlapping and subsequent obliteration of the bands is much retarded by the use of the prism; but the full development of the phenomenon demands that a should be finite. Let us inquire what is the condition in order that the width of the band of the n th order may be stationary, as λ varies. By (41) it is necessary that the variation of $\lambda^2/(\frac{1}{2}n\lambda - a)$ should vanish. Hence

$$2\lambda(\frac{1}{2}n\lambda - a) - \frac{1}{2}n\lambda^2 = 0,$$

or

$$a = \frac{1}{4}n\lambda. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (42)$$

The thickness of the plate where the n th band for λ is formed being $\frac{1}{2}n\lambda$, equation (42) may be taken as signifying that the thickness must be half due to curvature and half to imperfect contact at the place of nearest approach. If this condition be satisfied, the achromatism of the n th band, effected by the prism, carries with it the achromatism of a large number of neighbouring bands*.

We will return presently to the consideration of the spherically curved glasses used by Newton, and to the explanation of some of the phenomena which he observed; but in the meantime it will be convenient to state the theory of straight bands in a more analytical form.

Analytical Statement.

If the coordinate ξ represent the situation of the n th band, of wave-length λ , then, in any case of straight bands, ξ may be regarded as a function of n and λ , or, conversely, n (not necessarily integral) may be regarded as a function of ξ and λ . If we write

$$n = \phi(\xi, \lambda), \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (43)$$

and expand by Taylor's theorem,

$$\begin{aligned} n - n_0 = & \frac{d\phi}{d\xi_0} \delta\xi + \frac{d\phi}{d\lambda_0} \delta\lambda + \frac{1}{2} \frac{d^2\phi}{d\xi_0^2} (\delta\xi)^2 + \frac{d^2\phi}{d\xi_0 d\lambda_0} \delta\xi \delta\lambda \\ & + \frac{1}{2} \frac{d^2\phi}{d\lambda_0^2} (\delta\lambda)^2 + \dots, \quad . \quad . \quad . \quad . \quad (44) \end{aligned}$$

* *Enc. Brit.*, Wave-Theory, p. 428 (1888).

where

$$n_0 = \phi(\xi_0, \lambda_0). \quad . \quad . \quad . \quad . \quad . \quad (45)$$

The condition for an achromatic band at ξ_0, λ_0 is

$$\frac{d\phi}{d\lambda_0} = 0; \quad . \quad . \quad . \quad . \quad . \quad (46)$$

and, further, the condition for an achromatic system at this place is

$$\frac{d^2\phi}{d\xi_0 d\lambda_0} = 0. \quad . \quad . \quad . \quad . \quad . \quad (47)$$

If these conditions are both satisfied, n becomes very approximately a function of ξ only throughout the region in question.

In several cases considered in the present paper, the functional relation is such that

$$n = \xi \cdot \psi(\lambda), \quad . \quad . \quad . \quad . \quad . \quad (48)$$

$\psi(\lambda)$ denoting a function of λ only. The expansion may then be written

$$n - n_0 = \xi \{ \psi(\lambda_0) + \psi'(\lambda_0)\delta\lambda + \frac{1}{2}\psi''(\lambda_0)(\delta\lambda)^2 + \dots \}. \quad (49)$$

The line $\xi=0$ is here of necessity perfectly achromatic. If there be an achromatic system,

$$\psi'(\lambda_0) = 0;$$

and when this condition is satisfied, the whole field is achromatic, so long as $(\delta\lambda)^2$ can be neglected.

If the width of the bands be a function of λ only, n is of the form

$$n = \xi \cdot \psi(\lambda) + \chi(\lambda), \quad . \quad . \quad . \quad . \quad . \quad (50)$$

more general than that just considered (48), though of course less general than (43). The condition for an achromatic line is

$$\frac{dn}{d\lambda} = \xi \psi'(\lambda_0) + \chi'(\lambda_0) = 0, \quad . \quad . \quad . \quad (51)$$

and for an achromatic system,

$$\frac{d^2n}{d\xi d\lambda} = \psi'(\lambda_0) = 0; \quad . \quad . \quad . \quad . \quad (52)$$

so that, for an achromatic system, ψ' and χ' must both vanish.

Curved Interference-Bands.

If the bands are not straight, n must be regarded as a function of a second coordinate η , as well as of ξ and λ . In the equation

$$n = \phi(\xi, \eta, \lambda), \quad . \quad . \quad . \quad . \quad . \quad (53)$$

if we ascribe a constant value to λ , we have the relation between ξ, η corresponding to any prescribed values of n —that is, the forms of the interference-bands in homogeneous light. If the light be white, the bands are in general confused; but those points are achromatic for which

$$\frac{dn}{d\lambda_0} = 0. \quad . \quad . \quad . \quad . \quad . \quad . \quad (54)$$

This is a relation between ξ and η defining a curve, which we may call the achromatic curve, corresponding in some respects to the achromatic line of former investigations, where n is independent of η . There is, however, a distinction of some importance. When n is a function of ξ and λ only, the achromatic line is also an achromatic band; that is, n remains constant as we proceed along it. But under the present less restricted conditions n is not constant along (54). The achromatic curve is not an achromatic band; and, indeed, achromatic bands do not exist in the same development as before. They must be regarded as infinitely short, following the lines $n = \text{constant}$, but existent only at the intersection of these with (54). Practically a small strip surrounding (54) may be regarded as an achromatic region in which are visible short achromatic bands, crossing the strip at an angle dependent upon the precise circumstances of the case.

The application of this theory to the observations of Newton presents no difficulty. The thickness of the layer of air at the point x, y , measured from the place of closest approach, is

$$t = a + b(x^2 + y^2); \quad . \quad . \quad . \quad . \quad . \quad (55)$$

and since $t = \frac{1}{2}n\lambda$, the relation of n to x, y , and λ is

$$\frac{1}{2}n = a\lambda^{-1} + b\lambda^{-1}(x^2 + y^2). \quad . \quad . \quad . \quad . \quad . \quad (56)$$

This equation defines the system of bands when the combination is viewed directly. The achromatic curve determined by (54) is

$$a + b(x^2 + y^2) = 0,$$

and is wholly imaginary if a and b are both positive and finite. Only when $a=0$, that is when the glasses touch, is there an achromatic point $x=0, y=0$.

When a prism is brought into operation, we may suppose that each homogeneous system is shifted as a whole parallel to x by an amount variable from one homogeneous system to another. If the apparent coordinates be ξ, η , we may write

$$\xi = x - f(\lambda), \quad \eta = y. \quad . \quad . \quad . \quad . \quad (57)$$

Using these in (56), we obtain as the characteristic equation

of the rings viewed through a prism,

$$n = \frac{a + b\{\xi + f(\lambda)\}^2 + b\eta^2}{2\lambda}. \quad . \quad . \quad . \quad (58)$$

The equation of the achromatic curve is then, by (54),

$$\{\xi + f(\lambda_0) - \lambda_0 f'(\lambda_0)\}^2 + \eta^2 = \lambda_0^2 \{f'(\lambda_0)\}^2 - a/b, \quad . \quad (59)$$

which represents a *circle*, whose centre is situated upon the axis of ξ .

If the glasses are in contact ($a=0$), the locus is certainly real, and passes through the point

$$\xi + f(\lambda_0) = 0, \quad \eta = 0;$$

that is, the image with rays of wave-length λ_0 of the point of contact ($x=0, y=0$). The radius of the circle is $\lambda_0 f'(\lambda_0)$, and increases with the dispersive power of the prism. The other point where the circle meets the axis,

$$x = 2\lambda_0 f'(\lambda_0), \quad y = 0,$$

marks the place where the bands, being parallel to the achromatic curve, attain a special development. It is that which we should have found by an investigation in which the curvature of the band-systems is ignored.

If a be supposed to increase from zero, other conditions remaining unaltered, the radius of the achromatic circle decreases, while the centre maintains its position. The two places where the circle crosses the axis are thus upon the same side of the image of $x=0, y=0$. When a is such that

$$a/b = \lambda_0^2 \{f'(\lambda_0)\}^2, \quad . \quad . \quad . \quad (60)$$

the circle shrinks into a point, whose situation is defined by

$$x = \xi + f(\lambda_0) = \lambda_0 f'(\lambda_0), \quad y = \eta = 0. \quad . \quad (61)$$

Since there are two coincident achromatic points upon the axis, the condition is satisfied for an achromatic *system*. By (60), (61),

$$a/b = x^2,$$

so that

$$t = a + bx^2 = 2a. \quad . \quad . \quad . \quad (62)$$

This is the same result as was found before (42) by the simpler treatment of the question in which points along the axis were alone considered.

If a exceed the value specified in (60), the achromatic curve becomes wholly imaginary*.

* Compare Mascart, *Traité d'Optique*, t. i. p. 435.

XXIX. *The Thermal Effect due to Reversals of Magnetization in Soft Iron.* By A. TANAKADATÉ, *Rigakusi* *.

[Plates VI.-VIII.]

THE following experiment on the measurement of the heat due to the reversals of magnetization in soft iron wire was carried out in the Physical Laboratory of the Glasgow University during the summer months of 1888. The method pursued is due to Sir William Thomson, in accordance with whose instructions the work was undertaken.

Cotton-covered soft iron wire, of .115 centim. diameter, was coiled upon a specially prepared wooden groove. The ends of the wire were insulated from each other; and the bundle was tied at several places with fine silk thread to keep its shape, and when the wooden frame was removed the coil of the wire was left in shape of an anchor-ring. The object of thus building the ring with insulated wire instead of solid mass was to reduce the Foucault current in the substance of iron when an alternating current is passed in the magnetizing coil.

The ring thus formed was now wound uniformly over with No. 16 silk-covered copper wire in two layers, a thermoelectric junction consisting of platinoid and copper being placed upon the outer surface of the ring before winding. The principal dimensions of the ring were as follows:—

Diameter of the soft iron wire	0.115 centim.
Number of turns of the soft iron wire in the ring	130
Total number of turns of copper wire in the magnetizing solenoid	177
Internal diameter of the ring	6 centim.
External diameter of the ring	10 "
Mean	8 "

Mean strength of magnetizing field due to 1 ampere through the coil

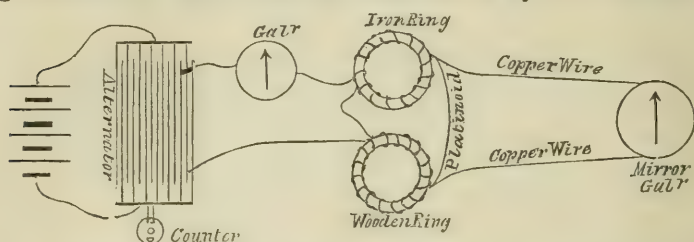
$$= 4\pi \frac{177}{8\pi \times 10} = 8.85 \text{ C.G.S.}$$

A wooden ring, of very nearly the same size and shape as that of the iron wire, was turned and wound similarly with the same silk-covered copper wire, total number of turns, however, being 174. The other junction of the same thermoelectric couple was laid on the surface of this wooden ring in the same way as the other junction was disposed with regard to the iron ring, so that the platinoid wire stretched between the two rings, and copper wire from the two rings led to terminals of a mirror-galvanometer, as in the diagram.

The magnetizing coils of the two rings were joined in series,

* Communicated by Sir William Thomson.

and led to an alternating-commutator through an idiostatic galvanometer. The alternator was driven by a clockwork.



It had forty teeth on each side, so that one revolution of it made forty complete reversals of magnetization in the iron ring inside the core. The current was supplied from secondary batteries, and its strength was adjusted by means of resistances, which were in most cases Edison lamps joined in multiple arc.

The thermoelectric constant of the junctions was determined by heating the iron ring in a specially prepared sand-bath, while the wooden ring was kept at the temperature of the room. The sand-bath was first heated by a gas-flame up to about 60°C ., and left to cool slowly. Several simultaneous readings of the galvanometer-deflexion and temperature-difference of the junctions were taken, and the value of the difference of temperature corresponding to one division of the galvanometer-deflexion was deduced as follows:—

Determination of Thermoelectric Constant (April 5, 1888).

Time.	Temp. of iron ring.	Temp. of wooden ring.	Difference of temperature.	Galvanometer-deflexion.	Difference of temperature corresponding to one division of galvanometer-deflexion.
h m					
12 0	53.6°C .	14.4°C .	39.2°C .	489	0.0802°C .
1 5	42.4	15.2	27.2	343	.0794
1 27	38.8	15.1	23.7	297	.0798
2 0	29.6	15.7	13.9	177	.0785
6 0	20.8	14.3	6.5	81	.0801
				Mean0796

From the time-rate of the fall of temperature of the iron ring we may judge of the uniformity of temperature inside the sand-bath. The constant was assumed to remain the same till the 20th of the same month, when a sudden change, due to changes in the arrangement of magnets in the laboratory, was observed. The constant was redetermined as follows:—

Determination of Thermoelectric Constant (April 20, 1888).

Time.	Temp. of iron ring.	Temp. of wooden ring.	Difference of temperature.	Galvanometer-deflexion.	Difference of temperature corresponding to one division of galvanometer-deflexion.
h m					
3 16	53.4 C.	16.2 C.	37.2 C.	458	0.0811
3 47	47.9	16.5	31.4	387½	0.0811
4 15	42.9	16.4	26.5	326	0.0814
5 2	36.3	16.3	20.0	245	0.0816
				Mean	0.0813

Hereafter the constant was tested every day by comparing the deflexion of the galvanometer caused by a permanent bar-magnet placed at a definite place.

The experiment consisted in making three different determinations, *i. e.*:—(1) the strength of the alternating current from which the magnetizing field is calculated; (2) the rate of alternation per second of time; (3) the rise of temperature of the junction at the iron ring above that at the wooden ring.

The alternating current was measured by a deci-ampere balance for small current, and by Siemens's dynamometer for large current. The dynamometer was carefully compared with the ampere-balance, and its constant was .878 ampere per division. The rate of alternation was determined by means of a counter connected to the commutator, and it varied between 28 and 400 complete reversals per second. The difference of temperature was observed by the mirror-galvanometer in the thermoelectric circuit. In most cases several observations were made during one experiment, so that the rise of temperature of the iron ring above that of the wooden ring can be graphically represented. Sometimes only the final readings were taken, omitting the intermediate points: these are distinguished from others by simply connecting the final point to the origin by a dotted line. The figures from 1 to 19 represent the experiment.

From the foregoing description of the method of measuring the difference of temperature, it appears that if the thermal qualities of the two rings are exactly the same, there will be no difference of temperature between the two junctions so long as heat is supplied from the current only, and hence the difference of temperature between the two junctions will indicate the heat due to the reversals of magnetization; in other words, Joule's effect will be entirely eliminated.

It was found, however, in the course of experiment that when a *continuous* current was passed through both the rings, the tendency of the temperature of the thermo-junction at the wooden ring was to rise above that at the iron ring, principally due to the difference of thermal diffusivity. But when the current was made to alternate, this was entirely overmasked by the heat arising from reversals of magnetization.

Thus, in order to find how much heat is due to the reversals of magnetization, a proper amount of allowance must be made for the heat diffused and radiated. A satisfactory way of arriving at the rate at which heat is generated in the substance of iron would be to solve the general equation of the conduction of heat when part of the conducting medium is generating heat. Such an equation will be

$$\frac{dv}{dt} = \kappa \nabla^2 v + q/c,$$

with proper boundary conditions as to time and space: v being the temperature, t time, κ diffusivity, q the rate of generation of heat per unit volume per unit time, c thermal capacity per unit volume.

Now when q is constant and the sole source of heat,

$$t=0, \quad v=0,$$

and therefore

$$\left(\frac{dv}{dt}\right)_{t=0} = q/c;$$

that is, the time-rate with which the temperature begins to rise is the true measure of the rate of generation of heat in the substance. As soon, however, as the elevation of temperature becomes sensible, heat will be diffused inside the body and radiated into the outside space. But whatever be the way in which heat is conducted in the body, when we consider the time-variation of temperature at a definite point in the body, the law of diffusion of heat will be expressed by

$$\frac{dv}{dt} = q - hv;$$

where h is a mixed coefficient depending upon conduction and emission, as the generating body is in contact with other conducting body as well as being exposed to the air. This mixed coefficient h might be called dissipativity, as it measures the rate at which heat is taken away from the generating body irrespective of how it is done.

Putting the single letter q for q/c for convenience, the above equation with the initial condition $t=0, v=0$, gives

$$v = \frac{q}{h}(1 - e^{-ht}).$$

This will be strictly applicable to the rise of temperature in

an infinitely thin wire through which current is made. Curves in fig. 21 give the rise of temperature thermoelectrically determined plotted against time, when a continuous current was sent through the copper coil of only one of the rings; the other being kept at the temperature of the room. They show how far the equation thus calculated is realized in experiment.

Expanding v in powers of t , thus

$$\begin{aligned} v &= \frac{q}{h} \left(1 - 1 + ht - \frac{h^2 t^2}{2} \dots \right) \\ &= q \left(t - \frac{ht^2}{2} + \frac{h^2 t^3}{3} \dots \right) \end{aligned}$$

we see that the time-rate of the rise of temperature at the beginning gives the rate of generation of heat independent of the dissipativity h , as was indicated by the general equation; and, therefore, whatever the diffusivities of the wooden and iron rings may be, the heating-effect due to current alone would have been eliminated in the beginning, if only the rate of generation of heat had been the same in both rings; in other words, the curve of temperature growth would have begun tangent to the time-axis, when the same continuous current was sent through both the rings. Experiment revealed, however, that this was not the case; the curve of difference of temperature growth began with definite rate (see fig. 22), so that there must have been a difference in the rate of supply of heat in the two rings. This is very likely due to the difference in the resistance of wire used in winding the rings, or irregularities in the rate of winding in the neighbourhoods of junctions.

Assume, therefore,

$$v_1 = q/h(1 - e^{-ht}) \text{ for the wooden ring,}$$

$$v_2 = q'/h'(1 - e^{-h't}) \quad ,, \quad \text{iron ring;}$$

taking their difference, we have

$$v = v_1 - v_2,$$

which is observed in the thermoelectric current. From this we have

$$\frac{dv}{dt} = qe^{-ht} - q'e^{-h't},$$

which shows that at the beginning the temperature rises at the rate $q - q'$ per unit of time; and, further, the difference of temperature, v , is maximum when

$$t = \log \left(\frac{q}{q'} \right) / (h - h');$$

and, therefore, there is or is not a maximum according as q and h are greater or less than q' and h' correspondingly or

not. In the present case $q > q'$, hence we infer that $h > h'$ from the existence of a maximum. The point of inflexion in the temperature-curve is given by

$$t = \log \left(\frac{qh}{q'h'} \right) / (h - h'),$$

which shows that when there is a maximum the point of inflexion takes place at a later period than the maximum.

The ultimate temperature when the current is kept running for a very long time is given by

$$v = q/h - q'/h';$$

and, therefore, we see that the curve does or does not cross the zero-line of temperature according as the ratio q/h is $<$ or $> q'/h'$. See fig. 20, where the temperature of iron ring above that of wooden ring is taken positively.

So much for continuous current, that is, when the supply of heat is derived solely from the magnetizing solenoid, which forms a kind of anchor-ring shell. Now, when an alternating current is passed, the whole mass of the iron ring becomes a source of heat, which is the subject of the study. The dissipativity h' will no longer be the same as before; for the whole mass of iron, instead of conducting away heat from the surface shell, becomes a source of heat, and therefore, when its rate of generating heat is greater than that due to the current it gives heat to the outer shell.

Put Q for the heat produced per unit time by reversals of magnetization, and H the dissipativity at the point where the thermoelectric junction is placed, then

$$v_2 = \frac{Q + q'}{H} (1 - e^{-Ht}) \text{ for the iron ring,}$$

$$v_1 = q/h (1 - e^{-ht}) \quad ,, \quad \text{wooden ring.}$$

Expanding v_1 and v_2 and subtracting,

$$v_2 - v_1 = v = (Q + q' - q)t - \{(Q + q')H - qh\} \frac{t^2}{2} + \dots$$

But q and q' will have a constant ratio, as they are both proportional to the square of current, so that we may put

$$q - q' = \alpha c^2,$$

and this is what is approximately true, as found by experiment (see fig. 22).

Also as h and H are constants, and $H < h$ and $q' < q$, from the foregoing investigation we may put

$$q'H - qh = -\beta c^2.$$

Thus the rise of temperature of the iron ring above the wooden one, when the same alternating current is passed

through both, can be expressed very approximately when time is sensibly small by

$$v = (Q - \alpha c^2)t - (QH - \beta c^2) \frac{t^2}{2} + \dots,$$

whence we see that the temperature-curve bends toward or away from the time-axis according as QH is greater or less than βc^2 . Now Q depends upon the rate of reversals of magnetization as well as on the magnitude of the magnetic hysteresis. Hence we see that the temperature-curve is turned upward when \mathfrak{H} , the magnetizing field, is large compared with the hysteresis, and *vice versa*. By referring to the figs. 1 to 20, one sees now why some of them are curved upward while others are curved downward. The downward curvature only occurs when the hysteresis is large compared with the field \mathfrak{H} , that is in the steepest part of the curve of magnetization.

In order to find Q from the observed data an empirical equation $v = At + Bt^2$ was assumed, and A was taken to be equal to $Q - \alpha^2 c$ for each curve, instead of determining the tangent to the curve from H *h* *q* *q'* *c*. The equation will be true if the heat generated were immediately communicated to the thermo-junction. But on account of the cotton covering, which lies between the iron and the junction, there will be a slight time-lag for the temperature-bore to reach the junction. To take this into account another constant is added to the equation thus

$$v = At + Bt^2 + C.$$

This C will give the temperature which the ring would have had at the beginning if there were no time-lag; or it might otherwise be looked upon as a constant error in zero of the galvanometer.

The constant A was found for each curve from the observed data by the usual method of least squares. It was found, however, in the course of calculation that when the number of observations was only 3 or 4, the three constants gave too much freedom to the curve; that is, the law of least squares is not nearly fulfilled by so few points. In other words, errors of observation rather tended to modify the form of the curve instead of compromising amongst themselves as would be the case when there are many. From this it was thought expedient to take the equation with only two constants, A, B , when there were less than 6 points (double the number of constants) in the curve, and three constants A, B, C , when there were more.

The tangent A thus found is now to be corrected for the differential Joule's effect, which was denoted by αc^2 . Fig. 22 shows the curves of temperature growth when a continuous current of given strength was sent through both the rings, and

curves (fig. 23) show the integral amount of temperature corresponding to different currents after given time has elapsed, the ordinate being the reading of the Siemens dynamometer, which is proportional to c^2 . The curves show that the difference of temperature due to Joule's effect is sufficiently expressed by the form αc^2 , where α is constant. From these curves the correction was found for each experiment, and αc^2 is drawn below the time-ordinate in each curve by dashes (figs. 1-19); and the true rate of the generation of heat arising from the reversals of magnetization is taken to be the algebraic difference of the ordinate of the tangent at origin, and this line. It is seen that this term αc^2 is so small, when \mathfrak{H} is less than 10, that the line in the diagram is scarcely distinguishable from the time axis.

When the field \mathfrak{H} was increased above 30 C.G.S. units, the heat due to the current alone was so great that a large part of the heat due to magnetic reversals was neutralized by it. Special experiments were made in which a continuous current was sent through both the rings for more than half-an-hour, when the time-variation of temperature became very small, and then the current was alternated. Fig. 20 shows three experiments of this kind. It is seen that in all the cases the effect of the continuous current was to heat the wooden ring; and when the current was alternated, the temperature of the iron ring rose suddenly almost in straight line. It must be remarked here that when the current was made to alternate its strength diminished, partly owing to induction and partly to the increase of contact resistance. In the curve corresponding to $\mathfrak{H} = 32.8$ the heating-effect due to current was made to remain the same by suddenly taking off some resistance in the circuit at the instant of commencing the alternations. In other two this was not done: the difference can be seen from the values of \mathfrak{H} given alongside the curves.

The rate of heating thus obtained was divided by the number of complete reversals per second to find the heat due to one complete reversal of magnetization. The temperatures so found are plotted against \mathfrak{H} in fig. 27, the number near the points gives the rate of alternations per second.

Ballistic Experiment.

In order to compare the heating-effect thus far obtained with the hysteresis in the same ring, the amount of magnetization was now investigated ballistically. For this purpose a secondary coil was wound uniformly over each of the rings, and the current induced by changes of magnetization in the iron ring due to small sudden changes of magnetizing field was measured by the throw of a ballistic mirror-galvanometer in the circuit. The secondary coil over the wooden ring was

joined in series with that of the iron ring in such a way that the induced currents due simply to the magnetizing field (\mathfrak{H}) in the two rings will neutralize each other in causing the ballistic effect. This was of course a mere matter of convenience, as the irreversible part of the cycle of magnetization,

$$\int_{-h}^h (4\pi\mathfrak{I} + \mathfrak{H})d\mathfrak{H}$$

depends solely upon \mathfrak{I} and not upon \mathfrak{H} .

The ballistic galvanometer was one of Sir William Thomson's design, used some years ago for investigating dynamical qualities of metals. The period of its swing was made longer by loading the aluminium frame of the needle. The proportionality of the throw was tested by slipping off suddenly some temporarily formed loops of moderately fine copper wire from a permanent bar-magnet. This showed that the throw varied according to the number of these loops throughout the extent of the scale as far as the accuracy of the experiment could allow.

The magnetizing field, that is the field due to the primary coils of the rings, was varied by small sudden steps by increasing the conductivity of the circuit. This was accomplished by putting a number of Edison lamps in the circuit, arranged as in the so-called Mho-box. The strength of the current was measured after each step by a deci-ampere balance in its circuit.

The value of the change of magnetization of the iron wire corresponding to one division of the ballistic throw was determined by putting an earth induction-coil in the galvanometer circuit and observing the throw when the coil was quickly turned. The coil was wound upon a circular wooden frame provided with proper axis: its dimensions were as follows:—

Diameter of the frame	30.5 centim.
Diameter of the coil after winding the wire	32.4 centim.
Number of turns of wire in the coil (8 layers of 27 turns each) . . .	216
Sum of areas of all the turns . . .	167640 sq. centim.
Resistance of the coil (at 11° C., room's temperature)	4.306 ohms.
Resistance of the galvanometer . .	.634 ohm.
Resistance of leading wires229 ohm.

So that, when the experiment with the earth induction was made, the circuit had the total resistance of . 5.169 ohms.

Horizontal intensity of the earth's field . . . 152 C.G.S.

Ballistic throw caused by one complete
revolution of the coil about a vertical axis

fixed in the plane of the coil 15 scale-divns.

This was mean of five observations agreeing amongst themselves within $\frac{1}{2}$ division.

Number of turns of the secondary coil

over the rings 100

Total area of the sections of iron wire

in the ring, $130 \times \frac{\pi}{4} (\cdot 115)^2$ = 1.35 sq. centim.

Resistance of the secondary coils 605 ohm.

Resistance of the galvanometer 634 ohm.

Resistance of the leading wire 229 ohm.

Whence the total resistance in the circuit

when the change of magnetization was

experimented upon 1.468 ohm.

From those numbers the value of \mathfrak{J} , the intensity of magnetization corresponding to one division of the ballistic throw is

$$\frac{167640 \times 2 \times \cdot 152}{15 \times 1.35 \times 100 \times 4\pi} \times \frac{1.468}{5.169} = \cdot 569 \text{ in C.G.S.}$$

Figs. 24, 25, 26 show the curves of magnetization when the magnetizing field \mathfrak{H} was cyclically varied between given limits. Each curve consists of two consecutive cycles; the points belonging to the first cycle are marked by dots, and those belonging to the second by crosses. The areas of those cycles $\int \mathfrak{J} d\mathfrak{H}$ were calculated by Simpson's method, and gave severally

$$\int_{-3.8}^{3.8} \mathfrak{J} d\mathfrak{H} = 1050 \text{ ergs} \quad \text{ I.}$$

$$\int_{-8.6}^{8.6} \mathfrak{J} d\mathfrak{H} = 6690 \quad ,, \text{ II.}$$

$$\int_{-21}^{21} \mathfrak{J} d\mathfrak{H} = 13610 \quad ,, \text{ III.}$$

The ring was now undone and 33.4 centim. of the iron wire was cut off, straightened and annealed, and then the curve of magnetization was studied by the magnetometric method. The result was anticipated in Phil. Mag. Nov. 1888, p. 450, in connexion with another research, where

$$\int_{-51}^{51} \mathfrak{J} d\mathfrak{H} = 18450 \text{ ergs.} \quad \text{ IV.}$$

Now if the whole work so done upon the substance of iron by cyclic change of magnetization be assumed to be used up

entirely in heating the iron, the rise of temperature will be obtained by dividing the number of ergs by Joule's equivalent and the heat-capacity of the iron per unit volume. Taking $\cdot 11$ for the specific heat, $7\cdot 7$ for the density, and $4\cdot 16 \times 10^7$ as Joule's equivalent, we have

$$1/(\cdot 11 \times 7\cdot 7 \times 4\cdot 16 \times 10^7) = 2^{\circ}\cdot 84 \times 10^{-8} \text{ C. per erg.}$$

Whence we get the rise of temperature corresponding to the above values:—

I.	. . .	$0\cdot 30 \times 10^{-4}$	C.
II.	. . .	1·89	„
III.	. . .	3·87	„
IV.	. . .	5·25	„

The temperatures thus calculated are plotted in fig. 27 in encircled points. It is there seen that the curve thus obtained lies above that obtained by direct thermometric measurement. The difference will of course be partly due to experimental imperfection, and partly to uncertainties in the assumed constants, and finally to the real cause, if there is any.

One apparent term neglected in the calculation, from the want of numerical result in literature, is the heat taken by the cotton covering of the iron wire. This cotton was found by weighing separately to be $\cdot 72$ per cent. of the iron by weight. The effect of introducing this correction will be slightly to lift up the points in the figure 27. But even if we assume its specific heat to be as much as that of water, it will not be enough to make up the difference.

Another point to be remembered is that the magnetizing field in the direct thermometric experiment was deduced from the square root of the readings of an idiostatic galvanometer, which gives the mean square of an alternating current; while in the ballistic and magnetometric experiments the field was directly determined. Now when the magnetization is effected gradually or by steps, the hysteresis depends upon the maximum field to which the iron is subjected (or the limits of cycle), so that in comparing the energy in hysteresis with the heating effect caused by alternating field, we ought to have taken the maximum fields in the course of alternations instead of the mean square root. It was found during the experiment that when an E.M.F. of 100 volts with high resistance was used, the current, when alternating, was little above 80 per cent. of what it was when the alternator was stopped. From this we may infer that the mean square root is not very far from the maximum value we are after; and if we imagine each point in the curve of heating observed in fig. 27 to be somewhat shifted towards the right, they would not be far from corresponding to what actually took place.

The effect of temperature in changing the magnetic quality of

the iron is entirely neglected, as it has been found by many experimenters to be very small when the temperature is less than 100° C. or so.

Putting aside these considerations for the time being, the experiment, imperfect as it is, seems to indicate that a large part (about 80 per cent.*) of the energy involved in hysteresis is used in heating the substance of iron; that the speed with which the cycle is performed has very little effect upon hysteresis, that is within 28 and 400 complete periods per second; that soft iron takes in $\frac{1}{400}$ sec. at least more than 70 per cent. of the magnetization it would have when subjected to the field for hours.

Two curves (fig. 28) are added, as calculated from Prof. Ewing's papers†, one for very soft iron and the other for tempered pianoforte steel. The curve for soft iron will be seen to lie lower than the observed temperature in the present experiment, and in the steel the heating is seen to be more than six times that for soft iron.

[A somewhat more accurate method of measuring this dissipated energy has been thought of, and approved by Sir William Thomson. It is hoped that the work will be undertaken before long and more satisfactory results arrived at.]

XXX. *Notices respecting New Books.*

Graphics, or the Art of Calculation by Drawing Lines, applied especially to Mechanical Engineering: with an Atlas of Diagrams.
By Prof. R. H. SMITH. Part I. (Longmans, 1889.)

THE author observes that "this book will not enable the student of Practical Mechanics to dispense with the use of other books treating mechanics in the ordinary manner." After reading his work, we say that this witness is true. What, then, is the writer's intention? "It [the book before us] is intended to enable those who have a knowledge of elementary mechanics to advance that knowledge to any degree of thoroughness they may find useful, and to apply that knowledge to the everyday problems of engineering science, without the aid of the more complicated portions of algebraic and trigonometrical Mathematics, or of the differential and integral calculus. Many have no taste or faculty for this latter sort of Mathematics: others have not the time needed to keep them *au fait* in its use; and, again, it is undeniably true that the solution of many a problem becomes practicable in point of time and ease by the graphic method which would be intolerably tedious and difficult without its aid." We remember hearing, some eighteen years since, Prof. Crofton explain some diagrams he had drawn in illustration of a paper on the "Stresses in Warren and Lattice

* Profs. Warburg and Hönig, by calorimetric method, find the directly observed heat to be from 60 to 70 per cent. of the amount calculated from hysteresis in bar-magnets. (Wied. *Ann.* 1883, p. 814.)

† Phil. Trans. part ii. 1885. The numbers for soft iron are taken from p. 556, and those for the steel calculated from the diagram in fig. 16 of pl. 60.

Girders." He then stated he had met with nothing to help him in English text-books, and his references were confined to the now well-known papers by Rankine* and Clerk-Maxwell†. It was at the same meeting that the late Mr. Merrifield and Prof. Henrici drew the attention of English mathematicians to a work then little known in this country, Culmann's *Calcolo Grafico* (1866), in which Prof. Crofton's constructions had been anticipated, and the methods applied to a wide range of subjects. It was at this same meeting (April 13, 1871) that Prof. Henrici illustrated the subject by a very simple and ingenious notation, which is now known as Bow's notation. In the Appendix to vol. iii. of the London Mathematical Society's Proceedings (pp. 320-2) Prof. Henrici has given a sketch of Culmann's work; now, such progress has been made on the Continent in the interim, this work itself is almost antiquated. In a Synopsis of Lectures on the Elements of Applied Mechanics‡ (p. 76) Prof. Crofton gives a short space to Culmann's method. We have a short notice of it also in Minchin, and more recently still in Lock's 'Elementary Statics.' Now and again we have seen questions in Cambridge Examination Papers requiring a knowledge of the art. To the craft of Engineers, of course, the method is familiar nowadays, but we give the above notes to show that it is gradually finding its way into more exoteric circles. A fine opportunity is open to some practised hand to bring these modern methods more fully before an English-speaking audience than has yet been done. Mr. Smith has had this opportunity, but we do not find in his book altogether what is wanted. As he has read, we presume, the recent works by Lévy, Favero, and Savolti, in addition to the earlier works by Culmann and Cremona, and possesses, we believe, the requisite manual skill—his book of plates which accompanies the text is most admirable, the constructions being most carefully drawn—there is still the opportunity open to him of supplying more effectually than he has done, in our opinion, in the work before us, the much wanted text-book. We have omitted to state that the student will find much interesting matter in Eagles's (why does Prof. Smith always quote him as "Eagle"?) 'Constructive Geometry of Plane Curves.'

We rapidly run through a list of the matters treated. In the first place the instruments required are described and their use explained; and then the subject is subdivided into Graph-Arithmetic, Graph-Algebra, Grapho-Trigonometry, Grapho-Dynamics, Grapho-Statics, Grapho-Kinematics, and then tabulations of Results, experimental and mathematical, are given. The last three chapters, in which there is a good deal of good work, discuss flat static structures without, and with, Beam Links and Solid Static Structures. In Part II. we are promised a treatment of the following subjects:—The distribution of Stress and Strain; the strength, stiffness, and design of Beams and Struts; Statics and Dynamics of Machines, Governors, Flywheels, and many such like important

* Cf. 'Papers,' pp. 562-4.

† "Reciprocal Figures and Stiff Frames," Phil. Mag. 1864, vol. xxvii.

‡ Hodgson (1877).

and interesting matters. The book is accompanied by a fair index and a very necessary glossary of terms and symbols; for our author has a fad, and some of his work reminds us of the Appendix to Dodgson's 'Euclid and his Modern Rivals,' in which those who know, know that there are many things hard to be understood.

XXXI. *Intelligence and Miscellaneous Articles.*

ON LEIDENFROST'S PHENOMENON. BY K. S. KRISTENSEN.

THE author proves that the heat conveyed from the heated dish to the drop is not sufficient to explain the Leidenfrost phenomenon, but that the heat transmitted by conduction of the vapour must also be taken into account. Taking the temperature of the dish at 200° , that of the drop at 100° , and assuming that each are perfectly black, and that the conductivity of aqueous vapour is equal to that of air, the quantities of heat transferred by conduction and radiation are equal, if the distance between the dish and the drop is 0.135 centim. As the assumptions for the radiation are too favourable, the quantity of heat transmitted by conduction certainly preponderates.—*Tidsskrift for Physik og Chemie* [2] ix. p. 161; *Beiblätter der Physik*, xiii. p. 155.

DETERMINATION OF THE CHANGE WHICH THE INTERNAL FRICTION OF AIR EXPERIENCES AT DIFFERENT TEMPERATURES AND UNDER VARIOUS PRESSURES. BY P. DE HEEN.

A brass tube 2 cm. wide and 85 cm. long, closed at both ends, carefully polished on the inside, is placed in an oil-bath, and a light brass cylinder 8.5 cm. in length is allowed to fall through. The diameter of the cylinder is about 0.35 mm. smaller than the clear width of the tube, so that the air on the fall of the cylinder must pass through an annular capillary slit. To the cylinder is attached an iron wire, so that it could be held up by an electromagnet; it was allowed to fall, and the time measured which elapsed before it struck against the bottom of the tube. Neglecting any possible friction of the cylinder against the sides of the tube, we have

$$\eta = T. \text{ const.},$$

where η is the coefficient of internal friction of air. The temperature was varied from 0° to 300° , the pressure of the air enclosed in the brass tube varied from 1 to 2280 cm. of mercury. In agreement with earlier observers with the exception of M. Hirn, the author finds that at the pressure of the atmosphere the internal friction of the air increases as the two thirds power of the absolute temperature.

The variation of friction with the temperature appeared to have a maximum at 300 mm. pressure. The dependence of the internal friction on the temperature between 10 and 78 mm. pressure, as follows from the author's experiments, may be very well expressed by the ratio required by Clausius' theory; that is, proportional to the root of the absolute temperature, which above 80 mm. pressure is no longer the case.—*Bull. Acc. Belg.* [3] xvi. p. 195; *Beiblätter der Physik*, xiii. p. 124.

THE
LONDON, EDINBURGH, AND DUBLIN
PHILOSOPHICAL MAGAZINE
AND
JOURNAL OF SCIENCE.

[FIFTH SERIES.]

OCTOBER 1889.

XXXII. *Viscosity of Solutions.* By R. F. D'ARCY, B.A.,
formerly Scholar of Gonville and Caius College, Cambridge.*

[Plate XI.]

THE following paper gives an account of experiments on the determination of the viscosity of solutions, especially as a means of investigating chemical changes going on in them.

The liquids experimented on were (1) solutions of sulphuric and acetic acids of different concentrations at different temperatures, (2) solutions of chrome alum, (3) solutions of calcium chloride in water, and ethyl and methyl alcohols.

Experiments on Sulphuric-acid Solutions.

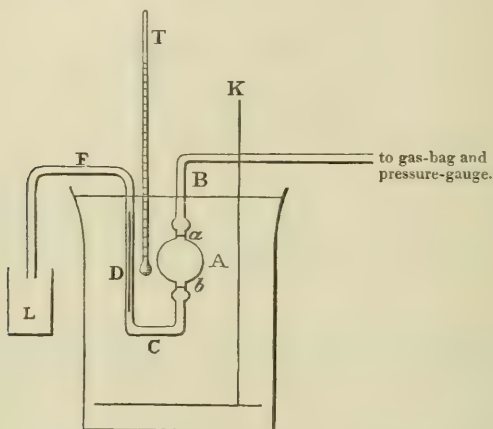
In 1861 Graham communicated a paper to the Royal Society ("On Liquid Transpiration in relation to Chemical Composition," Phil. Trans. 1861) containing the results of experiments he had made, following up Poiseuille's observation that a solution of alcohol corresponding to $C_2H_6O \cdot 3H_2O$ has maximum viscosity. One of the most striking observations recorded in this paper is that on adding water to sulphuric acid (which at $20^\circ C.$ is about twenty-one times more viscous than water) the viscosity increases, attaining a maximum when the water added is in the proportion of 18 to 98 of acid, that is to say, when the composition of the liquid is

* Communicated by the Author.

$\text{H}_2\text{SO}_4 \cdot \text{H}_2\text{O}$. As more water is added the viscosity decreases continuously. All Graham's experiments were made at a temperature of 20°C .

In the following experiments, by working at different temperatures, it is found that this increase of viscosity on the addition of water occurs at temperatures below about 65°C . : at higher temperatures the viscosity diminishes continuously as the concentration diminishes.

The apparatus used was very simple. It consisted of a bulb, A, which was connected by the tube, B, with a gas-bag



containing air under pressure, and to a water pressure-gauge, and by C to the fine tube, D. The capacity of the bulb A between the two scratches *a* and *b* is 15.9 c.c.

The experiments consisted in observing, by means of a stop-watch, the time taken to empty the bulb A of the liquid under examination through the tube D, the pressure being measured by means of the water-gauge, and the temperature by the thermometer, T. The water surrounding the bulb A and the tube D was kept at a uniform temperature by means of the stirrer, K.

No great accuracy was aimed at in these experiments ; the object being to establish in a general manner the alteration of the relation of viscosity to concentration at different temperatures, rather than the accurate determination of the viscosity of the different solutions used.

The acid used contained 2 per cent. of water. It was free from lead. Four solutions were used in addition to the undi-

luted acid ; these contained respectively 86·2, 83·3, 80·3, and 73·4 per cent. of H_2SO_4 . The viscosities of each of these were determined at temperatures varying between 10° and 100° .

The experimental data are given in the following Tables. By multiplying the numbers in the last column by ·027, the viscosity of the particular solution at the temperature of the experiment is obtained relatively to that of water at 20° . The numbers in the fourth column give approximately the effect of the siphon due to the end of the tube, F, being below the level of the centre of the bulb A.

TABLE I.—Viscosity of a solution containing 98 per cent. H_2SO_4 , 2 per cent. H_2O . (A.)

Mean temp.	Time. Seconds (t).	Pressure by gauge in mm. of water at 15° .	Siphon correction.	Total pressure (p)	$\frac{pt}{1000}$.
$14\cdot4$	2695	315·5	29	344·5	929
22·1	2020	316·5	29	345·5	698
27·5	1634	317	29	346	583
37·5	1257	317	28·5	345·5	434
45·5	1016	317	28·5	345·5	351
55	802	317·5	28·5	346	278
72	561	317·5	28·5	346	194
85	437	317·5	28·5	346	151
100·5	347	317·5	28	345·5	120

TABLE II.—Viscosity of a solution containing 86·2 per cent. H_2SO_4 , 13·8 per cent. H_2O . (B.)

Mean temp.	Time. Seconds (t)	Pressure by gauge in mm. of water at 15° .	Siphon correction.	Total pressure (p).	$\frac{pt}{1000}$.
$14\cdot4$	3107	317	28·5	345·5	1073
20·7	2392	317	28·5	345·5	826
28·2	1828	317	28·5	345·5	632
36·5	1386	316·5	28·5	345	478
45·5	1064	316·5	28	344·5	367
55	819	316·5	28	344·5	282
70	578	317	28*	345	199
100	335	317	27	344	115
85	428	317	27·5	344·5	147

TABLE III.—Viscosity of a solution containing 83·3 per cent. H_2SO_4 , 16·7 per cent. H_2O . (C.)

Mean temp.	Time. Seconds (<i>t</i>).	Pressure by gauge in mm. of water at 15°.	Siphon correction.	Total pressure (<i>p</i>).	$\frac{pt}{1000}$.
12°	3139	316·5	28·1	344·6	1080
25	1883	317	28	345	650
35·3	1322	317	28	345	456
47·2	929	317	27·5	344·5	320
56·5	740·5	317	27·5	344·5	255
66·3	588·5	317·5	27	344·5	203

TABLE IV.—Viscosity of a solution containing 80·3 per cent. H_2SO_4 , 19·7 per cent. H_2O . (D.)

Mean temp.	Time. Seconds (<i>t</i>).	Pressure by gauge in mm. of water at 15°.	Siphon correction.	Total pressure (<i>p</i>).	$\frac{pt}{1000}$.
14°	1846	299·5	151	450·5	831
27·5	1132	299·5	151	450·5	510
41·3	734	299·5	149	448·5	329
50·5	657·5	248·5	149	397·5	261
69·9	425	248	147·4	395·4	168
80·2	351·5	248·5	146·5	395·0	139
90	294	248·5	145·5	394	116
100	254	248·5	144·5	393	100

TABLE V.—Viscosity of a solution containing 73·4 per cent. H_2SO_4 , 26·6 per cent. H_2O . (E.)

Mean temp.	Time. Seconds (<i>t</i>).	Pressure by gauge in mm. of water at 15°.	Siphon correction.	Total pressure (<i>p</i>).	$\frac{pt}{1000}$.
11·4	1494	330	26	356	532
23·4	1016	330	26	356	362
36·4	717·5	330·5	26	356·5	256
50·3	521	330·5	26	356·5	186
59·8	431	331	25·5	356·5	154
72·8	339	331	25·5	356·5	121
88·5	262·3	331·5	25	356·5	94
100·5	223	331·5	25	356·5	79·5

The results of the experiments recorded in the Tables are shown graphically in the curves marked fig. 1 (Plate XI.), the ordinates of points of which are given by the numbers in the last column (less 75); the abscissæ being the temperatures given in the first column.

At first the most viscous solution is that denoted by B; the others follow in the order C, A, D, E. The A curve crosses that of C at about 37° , and that of B at about 65° , so that at temperatures higher than this the order of the viscosities of the solutions is the same as that of their concentrations.

In fig. 2 the coordinates are viscosity and concentration, expressed by the amount of water added to 100 of H_2SO_4 and the viscosities of the different solutions, at intervals of ten degrees, are marked. Definite curves are not drawn at the lower temperatures, but the different points corresponding to the same temperature are joined by straight lines. If more solutions had been used curves might have been drawn throughout in this diagram, which would then have been much more satisfactory.

These results may be explained as being due to the increasing dissociation of the hydrate $\text{H}_2\text{SO}_4 \cdot \text{H}_2\text{O}$ as the temperature rises. It is noteworthy that these experiments show, as far as they go, that the viscosity of solutions of sulphuric acid varies with the concentration and temperature in the same kind of way as the electrical resistance as determined by Kohlrausch.

For subsequent experiments an improved apparatus was fitted up suitable for liquids less viscous than those previously used. The capacity of the bulb of the new apparatus was 52.5 cub. centim. The beaker of water, by which the bulb and capillary tube were kept at the required temperature, was in these experiments jacketed by being put in a box and surrounded with cotton waste. Two apertures, in opposite sides of the box, enabled the bulb to be seen. The pressures were measured by means of a water-gauge, the height of which was observed by a cathetometer.

Experiment on Acetic-acid Solutions.

A similar set of experiments was made on solutions of acetic acid, but more solutions were used. The viscosity of each was observed at temperatures varying between 20° and 80° . Some experiments were tried at 100° , but they are not recorded here since they were vitiated to a great extent, owing to the formation of bubbles in the bulb and tube. The experimental numbers are given in Tables VI. to XVII. From these numbers a set of viscosity-temperature curves

were drawn, similar to those given in fig. 1 for sulphuric acid. The curves given in fig. 3 are derived from these just as those in fig. 2 are obtained from fig. 1.

These curves show clearly that the hydrate is not completely dissociated in the solutions used at these temperatures, this is in accordance with the difficulty experienced in separating water from acetic acid by distillation.

Acetic-acid Solutions.

TABLE VI.—Solution I. 99.1% $\text{C}_2\text{H}_4\text{O}_2$, .9 H_2O .

Temperature.	Time. Seconds (<i>t</i>).	Pressure. Cm. of water at 20° (<i>p</i>).	$p \times t$ 1000	Viscosity. Water at 20°=1.
21°	867	32.48	28.16	1.21
29.7	757.5	32.48	24.60	1.06
39.4	658	32.48	21.37	.921
57.3	528.5	32.48	17.16	.740
49.4	578	32.53	18.80	.810
67.3	471	32.59	15.35	.662
82.5	403	32.60	13.14	.566

TABLE VII.—Solution II. 93.8 $\text{C}_2\text{H}_4\text{O}_2$, 6.2 H_2O .

Temperature.	Time. Seconds (<i>t</i>).	Pressure. Cm. of water at 20° (<i>p</i>).	$p \times t$ 1000	Viscosity. Water at 20°=1.
20°	1497.5	29.61	44.36	1.91
29.9	1207.5	29.61	35.76	1.54
39.4	1009	29.57	29.82	1.28
49.5	843.5	29.57	24.93	1.07
58.1	735	29.57	21.73	.915
69.5	622.5	29.60	18.43	.794
82.2	531.5	29.60	15.73	.678

TABLE VIII.—Solution III. 88.24 $\text{C}_2\text{H}_4\text{O}_2$, 11.76 H_2O .

Temperature.	Time. Seconds (<i>t</i>).	Pressure. Cm. of water at 20° (<i>p</i>).	$p \times t$ 1000	Viscosity. Water at 20°=1.
20.1	1938	28.79	55.81	2.41
29.85	1518	28.86	43.8	1.89
39.5	1222	28.88	35.3	1.52
49.9	995.5	28.88	28.75	1.24
59.5	841.5	28.91	24.33	1.05
76.2	682.5	28.87	18.84	.812
65.7	766	28.87	22.12	.953

TABLE IX.—Solution IV. 83.33 C₂H₄O₂, 16.67 H₂O.

Temperature.	Time. Seconds (<i>t</i>).	Pressure. Cm. of water at 20° (<i>p</i>).	$p \times t$ 1000	Viscosity. Water at 20°=1.
22.7	1706	32.67	55.71	2.40
30.2	1617	28.79	46.55	2.01
40.3	1273	28.78	36.64	1.58
50.1	1043	28.78	30.01	1.29
59.8	877	28.78	25.24	1.09
71.9	720	28.87	20.79	.894
79	648	28.87	18.71	.807

TABLE X.—Solution V. 81.08 C₂H₄O₂, 18.92 H₂O.

Temperature.	Time. Seconds (<i>t</i>).	Pressure. Cm. of water at 20° (<i>p</i>).	$p \times t$ 1000	Viscosity. Water at 20°=1.
20	1844	33.13	61.08	2.63
30	1417	33.12	46.92	2.02
40	1114	33.24	37.01	1.59
48.5	933	33.26	31.03	1.34
61.3	741.5	33.26	24.66	1.06
72.5	620	33.26	20.62	.889
78.1	570	33.16	18.90	.815

TABLE XI.—Solution VI. 78.95 C₂H₄O₂, 21.05 H₂O.

Temperature.	Time. Seconds (<i>t</i>).	Pressure. Cm. of water at 20° (<i>p</i>).	$p \times t$ 1000	Viscosity. Water at 20°=1.
20.2	1836	33.12	60.81	2.62
29.9	1421	33.13	47.08	2.03
39.5	1131.5	33.14	37.52	1.62
49.7	917	33.15	30.40	1.31
59.4	771	33.16	25.57	1.11
67.7	671.5	33.16	22.26	.96
78.3	570	33.16	18.90	.815

TABLE XII.—Solution VII. 76.92 C₂H₄O₂, 23.08 H₂O.

Temperature	Time. Seconds (<i>t</i>).	Pressure. Cm. of water at 20° (<i>p</i>).	$p \times t$ 1000	Viscosity. Water at 20°=1.
20.1	1848.5	32.87	60.77	2.62
29.6	1436	32.86	47.19	2.03
40.1	1120	32.86	36.81	1.59
49.5	921	32.86	30.27	1.31
59.6	766.5	32.86	25.19	1.09
69.7	649.5	32.86	21.34	.92
81.2	545	32.86	17.91	.772

TABLE XIII.—Solution VIII. 75 C₂H₄O₂, 25 H₂O.

Temperature.	Time. Seconds (<i>t</i>).	Pressure. Cm. of water at 20° (<i>p</i>).	$\frac{p \times t}{1000}$	Viscosity. Water at 20°=1.
20·7	1813	32·89	59·64	2·57
30·3	1403·5	32·89	46·17	1·99
39·4	1130	32·89	37·17	1·60
49·8	909	32·89	29·90	1·29
57·8	783	32·89	25·75	1·11
68	659	32·89	21·68	·934
84	519	32·89	17·07	·736

TABLE XIV.—Solution IX. 73·17 C₂H₄O₂, 26·83 H₂O.

Temperature.	Time. Seconds (<i>t</i>).	Pressure. Cm. of water at 20° (<i>p</i>).	$\frac{p \times t}{1000}$	Viscosity. Water at 20°=1.
19·8	1845	32·78	60·47	2·61
29·9	1408	32·78	46·13	1·99
40·1	1106	32·78	36·24	1·56
49·3	914	32·76	29·93	1·29
58·5	772	32·74	25·27	1·09
68·4	655	32·71	21·41	·925
81·7	536	32·68	17·52	·755

TABLE XV.—Solution X. 68·19 C₂H₄O₂, 31·81 H₂O.

Temperature.	Time. Seconds (<i>t</i>).	Pressure. Cm. of water at 20° (<i>p</i>).	$\frac{p \times t}{1000}$	Viscosity. Water at 20°=1.
19·6	1813·5	32·91	56·69	2·57
29·5	1395	32·91	45·91	1·98
40	1084·5	32·91	35·72	1·54
50·2	881	32·88	28·97	1·25
59	747	32·88	24·57	1·06
70·1	621	32·88	20·42	·88
84·9	502	32·88	16·51	·711

TABLE XVI.—Solution XI. 62·5 C₂H₄O₂, 37·5 H₂O.

Temperature.	Time. Seconds (<i>t</i>).	Pressure. Cm. of water at 20° (<i>p</i>).	$\frac{p \times t}{1000}$	Viscosity. Water at 20°=1.
19·9	1697·5	32·82	55·73	2·40
30·5	1275·5	32·82	41·89	1·81
40	1022	32·82	33·55	1·45
50·5	823·5	32·85	27·06	1·17
57·6	718·5	32·88	23·62	1·02
68·6	598·5	32·88	19·68	·848
78·7	517	32·88	17·00	·733

TABLE XVII.—Water.

Temperature.	Time. Seconds (<i>t</i>).	Pressure. Cm. of water at 20° (<i>p</i>).	$p \times t$ 1000	Viscosity. Water at 20° = 1.
21·4	687	32·62	22·42	·966
30·3	568·5	32·61	18·54	·799
40·7	467·5	32·58	15·24	·656
53·3	384·5	32·58	12·53	·540
60·1	353·5	32·62	11·53	·497
73·6	304	32·62	9·92	·427
78·3	290	32·62	9·46	·408

Arrhenius has recently (Phil. Mag. July 1889) made some remarks on Graham's paper above referred to. He appears to think the explanations given by Graham to be quite antiquated. [Graham's work is not forty years old yet, by the way, since it was published in 1861.] He cites, in particular, the example of alcohol, and inaccurately states that the viscosity is a maximum for a solution having the composition $C_2H_5OH \cdot 5H_2O$ instead of $C_2H_5OH \cdot 3H_2O$; the proportions, in fact, for which, as Mendelejeff has shown, the greatest contraction occurs on mixing. He states that the fact that a maximum of viscosity occurs on dilution cannot be used as evidence of a combination of the liquids. Now the strange thing is, if this be so, that in a series of experiments such as Graham's these maxima should occur in every case when the number of molecules of water and the number of molecules of the other liquid are in some simple ratio; that in the case of sulphuric acid the hydrate can be obtained in the solid state; that in other cases the solution of maximum viscosity is that for which the greatest contraction occurs.

Again, Arrhenius states that the reason the hydrate explanation has been "abandoned" is because it was found that the maximum varies with the temperature. The solutions used in the acetic-acid experiments, above described, were specially chosen to test this; no such change occurs within the limits of experimental accuracy in the case of this acid below 60° at any rate.

In cases where the maximum does vary, I do not think the hydrate theory need be abandoned; since, if such a solution be considered as a dissociating system, the mixture containing the largest percentage of hydrate at any temperature need not necessarily be that in which the components are mixed in the proportions in which they combine, but there will be for each acid &c. at any particular temperature a certain mixture (the case being analogous to that of a gas mixture) which will

contain a maximum percentage of hydrate, and this will be the solution having maximum viscosity.

Experiments on Chrome-Alum Solutions.

It is well known that a solution of chrome alum when heated to 70° undergoes a change which is easily traced by the colour changing from violet to green, and that on cooling the solution remains for a long time green, but gradually returns to its original state. Experiments were made on the viscosity of a solution before and after being heated; the results are given in the following Table, and shown graphically in fig. 4:—

TABLE XVIII.—Viscosity of $\frac{7}{10}$ normal solution of Chrome Alum.

Mean Temp.	Time. Seconds (<i>t</i>).	Pressure. Cm. of water at 15° (<i>p</i>).	$\frac{pt}{1000}$.	Viscosity. Water at $15^{\circ}=1$.	Colour of sol.
16 $^{\circ}$	1132	28.42	32.0	1.245	violet
16.2	1115	28.42	31.7	1.224	violet
29.8	817	28.42	23.2	.896	violet
49.3	567	28.42	16.05	.620	violet
58.8	483	28.42	13.67	.528	colour changing
69.4	413	28.42	11.70	.452	green
49.35	528	28.42	15.0	.579	green
29.85	753	28.42	21.4	.826	[had been heated to 100° .]
16.05	1031	28.42	29.3	1.132	green
					green

Experiments were also made with a solution which was saturated in the cold with similar results.

This change of colour has been explained in two ways: (1) that it is due to dehydration; (2) more recently and perhaps more satisfactorily as due to the decomposition of the normal chromium salt with formation of basic and acid salts. The diminished viscosity may perhaps be taken to indicate a decomposition of complex molecules into simpler ones, but does not seem capable of discriminating between these two theories. Probably osmotic pressure being chiefly dependent on the number of molecules in solution would yield a crucial method of experiment from the physical point of view.

The alteration of viscosity is so marked in this case that it would be sufficient to show that a change had been produced by heating; hence it seems that in cases where there is no alteration of colour determinations of viscosity may be used

to detect a change which otherwise might pass, or, perhaps, has passed, unnoticed.

Experiments on Solutions of Calcium Chloride.

These were undertaken with a view to investigate the viscosities of solutions of the same salt in different solvents. For this purpose viscosities of solutions of calcium chloride* [$\frac{1}{5}$, $\frac{1}{10}$, $\frac{1}{20}$ normal] in water, ethyl alcohol, and methyl alcohol were determined. Considerable trouble was taken to purify the substances used. As no obvious relation appears to exist an account of only a few of the experiments is given in the following Table. The noteworthy result of these experiments is that the increase of viscosity on adding calcium chloride to either of the alcohols is much greater than that produced when it is added to water. Perhaps the explanation of this is to be found in the superior dissociating action of the water.

TABLE XIX.

Liquid.	Temperature.	Viscosity. Water = 1.	Differences.	Temperature.	Viscosity.	Differences.
$\frac{1}{5}$ CaCl ₂ in water Water.....	15 ⁰	1.053		50 ⁰	.543	
	15	1.		50	.513	
$\frac{1}{5}$ CaCl ₂ in C ₂ H ₆ O C ₂ H ₆ O	15	1.533	.053	50	.767	.030
	15	1.168		50	.621	
$\frac{1}{5}$ CaCl ₂ in CH ₄ O CH ₄ O	15	.768	.365	50	.466	.146
	15	.594		50	.381	
			.174			.085

The solutions used in these experiments, with the exception of the sulphuric-acid solutions, were made up in the chemical laboratory of Gonville and Caius College. The viscosity determinations were made in the Cavendish Laboratory. My thanks are due to Professor Thomson for many valuable suggestions during the course of the work.

Gonville and Caius College,
Cambridge, July 1889.

* Fitzpatrick determined the electrical resistances of solutions of these and other strengths. B. A. Rep. 1886; Phil. Mag. Nov. 1887.

XXXIII. *Notes on Geometrical Optics.*—Part I.

By Professor SILVANUS P. THOMPSON, D.Sc.*

1. *The Deduction of the Elementary Theory of Lenses and Mirrors from Wave Principles.*

INTRODUCTORY.—The division of Optics into a “geometrical” part, founded upon the treatment of the subject from the ray point of view, and a “physical” part, founded upon the treatment of the subject from the wave point of view, has long seemed illogical. Experience in the teaching of the science suggests that it would be preferable to adopt the wave theory as a common basis, provided the formulæ of lenses and mirrors which form the staple of geometrical optics are as readily established on wave principles as on ray principles. Five years ago the author of these notes made an attempt to rewrite the elementary part of geometrical optics on wave principles; and though hitherto he has published nothing on the subject, he has subjected the method to the test of experience, and has made it the basis of his optical lectures year by year. The form finally adopted, and now for the first time set forth, has been modified from time to time and simplified. As a result, whilst the formulæ for second approximations, aberration and the like, can be deduced with no greater difficulty than in the orthodox way, the elementary formulæ for first approximations are much more simply deduced, and do not even require the use of trigonometrical functions. This is the more striking because, as all teachers of the subject know, the very first assumptions made in establishing the elementary formulæ of spherical mirrors and lenses are that angles may be written instead of their sines or tangents. The assumptions made in the method now published are, it is true, different, but involve no greater sacrifice of accuracy, the formulæ being true to the same degree of approximation, and, indeed, though written in a different notation, are in substance identical with those in common acceptance.

It will be convenient, first, to set forth the general bases of the method; next to define the method of reckoning curvatures; then to explain the notation employed; lastly, to develop the elementary formulæ.

2. *General Bases.*

In treating optics from the new standpoint, we have to think about surfaces instead of thinking about mere lines.

* Communicated by the Physical Society; read June 8, 1889.

Waves march always at right angles to their surfaces; a change in the form of the surface alters the direction of march. The wave-surface is to be considered instead of the "ray." The curvature of the surface therefore becomes the all-important consideration. All that any lens or mirror or any system of lenses or mirrors can do to a wave of light is to imprint a curvature upon the surface of the wave. If the wave is initially a plane wave, then the curvature imprinted upon it by the lens or mirror will result in making it either march toward a point (a real focus) or march as from a point (a virtual focus). If the wave possesses an initial curvature, then all that the lens or mirror can do is to imprint another curvature upon its surface, the resultant curvature being simply the algebraic sum of the initial and the impressed curvatures. As will be seen, in the new method the essential thing to know about a lens or mirror is the curvature which it can imprint on a plane wave: this is, indeed, nothing else than what the opticians call its "power;" the focal power being inversely proportional to the so-called focal length. Another, but less vital point in the method, is the abandonment of the use of the so-called index of refraction in favour of a quantity reciprocally related to it, and here denominated the velocity-constant. The use of the index of refraction dates from a time anterior to the discovery that refraction was a mere consequence of the difference of velocity of the waves in different media. The index of refraction is a mere ratio between the sines (or originally the cosecants) of the observed angles of incidence and refraction. The uselessness of clinging to it as a foundation for lens formulæ is shown by the simple fact that, in order to accomplish the very first stage of reasoning in the orthodox way of establishing the formulæ, we abandon the sines and write simply the corresponding angles, as Kepler did before the law of Snell was discovered. The elementary formulæ of lenses are, in fact, where Kepler left them. It is now common knowledge that the speed of light, on which refraction depends, is less in optically dense media than in air. The speed of light in air is not materially different from one thousand million feet per second, or thirty thousand million centimetres per second. If we take the speed of light in air as unity, then the numeric expressing the speed in denser media, such as glass or water, will be a quantity less than unity, and will differ for light of different wave-lengths. It is here preferred to take the speed of light in air, rather than *in vacuo*, as unity, because lenses and optical instruments in general are used in the air. The numeric expressing the

relative velocity in any medium is called its "velocity-constant"; it is the reciprocal of the index of refraction. The velocity-constant, for mean (yellow) light, for water is about 0.75; that of crown glass 0.65; that of flint glass from 0.61 to 0.56, according to its density.

3. Method of Reckoning Curvature.

The Newtonian definition of curvature as the reciprocal of the radius has a special significance in the present method of treating optics: for some of the most important of lens and mirror formulæ consist simply of terms which are reciprocals of lengths, that is to say of terms which are curvatures. The more modern definition of curvature as rate of change of angle per unit length of the curve (Thomson and Tait's 'Natural Philosophy,' ii. p. 5) is equivalent to Newton's; for if in going along an arc of length δs , the direction changes by an amount $\delta\theta$, the curvature is $\delta\theta/\delta s$. But the angle $\delta\theta = \delta s/r$, where r is the radius of curvature; hence the curvature $= \delta s/r\delta s = 1/r$.

There is, however, another way of measuring curvature, which, though correct only as a first approximation, is eminently useful in considering optical problems. This way consists in measuring the bulge of the arc subtended by a chord of given length.

Consider a circular arc AP, having O as its centre. Across this arc draw a chord PP' of any desired length. The diameter AB bisects it at right angles in M. The short line MA measures the depth of the curve from arc to chord. If the radius is taken as unity the line MA is the versed-sine of the angle subtended at B by the whole chord, or is the versed-sine of the semi-angle subtended at the centre. In Continental works it is frequent to use the name *sagitta* for the length of this line MA; and as this term is preferable to versed-sine, and can be used generally irrespective of the size of radius, it is here adopted. The proposition is that, for a given chord, the *sagitta* is (to a first degree of approximation) proportional to the curvature. For it follows from the construction that

$$MA \cdot MB = (PM)^2;$$

assuming PM as unity,

$$MA = \frac{1}{MB} = \frac{1}{2r - AM}.$$

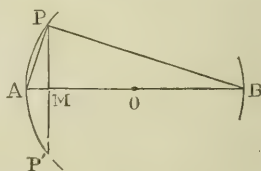


Fig. 1.

But, for small apertures, AM is small compared with $2r$, and may be neglected in the denominator, whence

$$MA \doteq \frac{1}{2} \cdot \frac{1}{r}.$$

Twice* the sagitta represents numerically the curvature. The error is less than 1 per cent. when the semi-angle subtended at the centre is 10° ; less than two per cent. when it is 15° ; less than five per cent. when it is 25° .

If the method of reckoning curvatures by means of the sagitta required justification, that is afforded by the fact that the practical method of measuring the curvatures of lenses and mirrors by the *spherometer* consists essentially in applying a micrometer-screw to measure the sagitta of the arc subtended by a fixed chord, the diameter of the contact circle drawn through the three feet of the instrument. In this case, as indeed in all cases where accuracy, not approximation, is desired, the basis for calculation of the correction exists in the actual size of the diameter of the contact circle, which is a fixed parameter for all measurements made with the instrument.

The sign of the curvature remains to be defined. In the case of actual waves of light, the sign adopted will be $+$ for the curvature of waves which are converging upon a real focus; $-$ for those which are diverging either from a luminous source or from a virtual focus. This agrees with the practice of the ophthalmists and of the opticians, who always describe a converging lens as positive. A positive lens is one which imprints a positive curvature upon a plane wave which traverses it.

The *unit of curvature*, whether of the wave-surface itself or of the surface of any mirror or lens, will be taken so as to accord with modern ophthalmic and optical practice as *the dioptrie*; that is to say, the curvature of a circle of one metre radius will be taken as unity. The dioptrie, originally proposed by Monoyer as the unit of focal power of a lens, was formally adopted in 1875 by the International Medical Congress at Brussels, and its great convenience has led to its universal adoption for the enumeration of the focal powers of lenses. That lens which has a focal length of 1 metre is said to have a focal power of one dioptrie. In other words, such

* Though the sagitta is numerically *half* the curvature, since all the formulæ of first approximation are homogeneous and of the first degree as regards sagittæ and curvatures, the numerical factor $\frac{1}{2}$ disappears in passing from sagittæ to curvatures, or *vice versa*.

a lens prints a curvature of one dioptrie upon a plane wave which is incident upon it. For the present proposal to extend the use of the term from focal powers (*i. e.* imprinted wave-curvatures) to the curvatures of curved surfaces in general, the writer is responsible.

4. Notation.

In adopting a notation which embodies the new method it is obviously advisable to choose one which lends itself most readily to the existing and accepted notations. In the great majority of books on optics, the recognized symbol for focal length is f ; that for radius of curvature r . And in the Cambridge text-books for many years the distances from lens or mirror of the point-object and the point-image have respectively been designated by the letters u and v . Now it is the reciprocals of these which occur in the expressions for the curvatures of surfaces or of waves. The symbols adopted respectively for the four reciprocals are accordingly \mathcal{F} , \mathcal{R} , \mathcal{U} , and \mathcal{V} . The accepted symbol for the index of refraction is the Greek letter μ ; for the velocity-constant, which is its reciprocal, we take the letter h . The following is a tabular statement of the symbols and their meanings:—

Symbol.	Meaning.	Equivalent in Current Notation.
\mathcal{F}	Focal curvature, or Focal power of lens or mirror (= dioptries, if metre is taken as unit of length)	$\frac{1}{f}$
\mathcal{R}	Curvature of Surface.....	$\frac{1}{r}$
\mathcal{U}	Curvature of Incident wave; <i>i. e.</i> curvature which it has acquired by having travelled from point of origin ("incident focus") to incidence.....	$\frac{1}{u}$
\mathcal{V}	Curvature of Resultant wave; <i>i. e.</i> curvature with which wave emerges from the lens.....	$\frac{1}{v}$
h	Velocity-Constant of medium; <i>i. e.</i> velocity of light in that medium compared with velocity in air taken as unity	$\frac{1}{\mu}$

Expansion of Curvatures.—If the curvature \mathcal{R} of a wave at any point is known it is easy to calculate the curvature at

any other point at distance d further from or nearer to the centre, the formula for the new curvature \mathcal{R}' being as follows:—

$$\mathcal{R}' = \mathcal{R} \frac{1}{1 \pm \mathcal{R}d} \cdot \cdot \cdot \cdot \cdot \cdot (1)$$

The + sign must be taken where the new point is further from the centre than the point for which the curvature \mathcal{R} is specified; the — sign when it is nearer the centre. This proposition is of use in dealing with thick lenses, and with thin lenses at a given distance apart.

5. Refraction Formulæ.

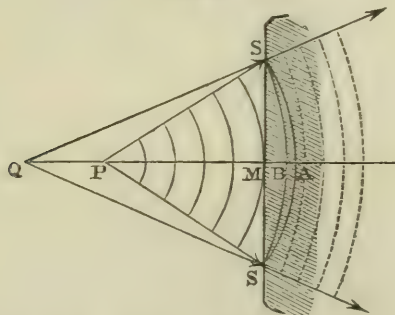
As a preliminary to lens formulæ, it is convenient to consider certain cases of refraction.

A. PLANE SURFACE: CURVED WAVE.

Case (i.) a. *Entrant Wave of Negative Curvature.*

Consider a retarding medium, such as glass, bounded on the left (fig. 2) by a plane surface SS. Let P be a source of

Fig. 2.



waves incident on the surface, PM being a line perpendicular to SS. The wave-fronts, at successive small intervals of time, are represented by arcs of circles. At a certain moment the wave, had it been going on in air, would have had for its surface the position SAS; the curvature being measured by the sagitta AM. The medium, however, retards the wave, and it will only have gone as far as B instead of penetrating to A; B being a point such that $BM = h$. AM, where h is the velocity-constant of the medium into which the wave enters. The curvature of the wave is flattened as the result of the retardation. Now draw a circle through SBS, and find its

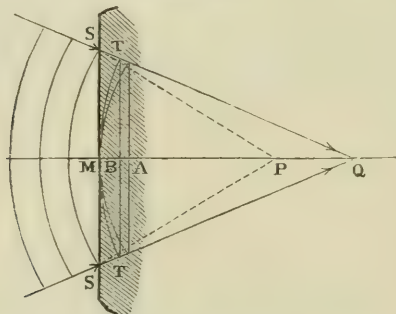
centre Q. To a first degree of approximation the arc SBS represents the retarded wave-front, the set of wave-fronts from B onwards being represented by the series of arcs drawn from Q as centre. An eye situated in the medium on the right of SS will perceive the waves as though coming from Q, the (virtual) point-image of P. Accurately the wave-fronts should be hyperbolic arcs, but if SS is small relatively to PM the circular arcs are adequate. Now $AM = \mathcal{U}$, and $BM = \mathcal{V}$. Hence the action of the plane surface upon the curvature of the incident wave is given by the formula

$$\mathcal{V} = h\mathcal{U}. \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

Case (i.) *b. Entrant Wave of Positive Curvature.*

The entrant wave (fig. 3) has a positive curvature or convergence such as would cause it to march to the point P (the

Fig. 3.



incident focus) if its path lay wholly in air. At a certain moment, when the middle point of the wave-front has reached M, the outer portions of the wave-front passing in through SS would have reached positions as far as the vertical line drawn through A had the path lain wholly in air. But being retarded they only reach as far as the line TBT drawn through B; where again $BM = h \cdot AM$. The circular arc through TBT has Q for its centre; that is to say, after entry the waves now converge on Q instead of P. In this case also the effect on the wave of entrance into the retarding medium is to flatten its curvature, and the formula is as before,

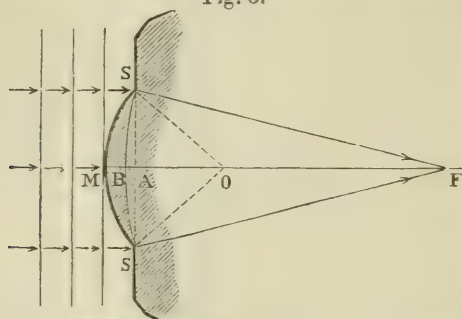
$$\mathcal{V} = h\mathcal{U}. \quad . \quad . \quad . \quad . \quad . \quad . \quad (2) \text{ bis.}$$

Case (ii.) *a. Emergent Wave of Negative Curvature.*

Consider the wave emerging (fig. 4) into air from a point

As a result the wave is imprinted with the curved surface SBS , the centre of which is at F , toward which point the

Fig. 5.



wave converges. The sagitta AB measures the focal curvature thus impressed on the wave. Now

$$\begin{aligned} AB &= AM - BM \\ &= AM - h \cdot AM, \end{aligned}$$

or

$$\mathcal{F} = \mathcal{R} (1 - h). \quad \dots \dots \dots (4)$$

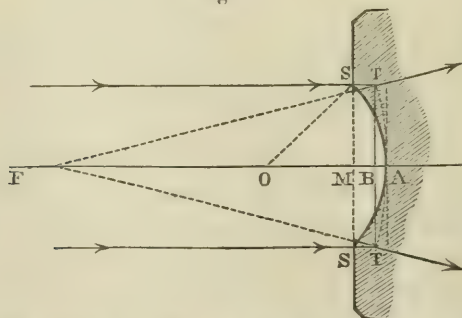
It will be noted that if the curvature of the surface is positive (*i.e.* bulging toward the source of light), the impressed focal curvature is also positive. For the case of any two media having respective velocity-constants h_1 and h_2 , the formula becomes

$$\mathcal{F} = \mathcal{R} \frac{h_1 - h_2}{h_1}. \quad \dots \dots \dots (5)$$

Case (i.) *b. Entrant Wave; Concave Surface.*

Consider the plane wave which would have reached the

Fig. 6.



vertical line though A had its path lain wholly in air; the central portion does reach A , but the marginal portions are

retarded, and only reach the vertical line T B T drawn through B, a point such that $BM = h \cdot AM$. The impressed curve is T A T with centre F. That is to say, the concave (or negative) surface imprints a negative focal curvature on the wave, its sagitta being A B.

$$\begin{aligned} AB &= AM - BM \\ &= AM - h \cdot AM, \end{aligned}$$

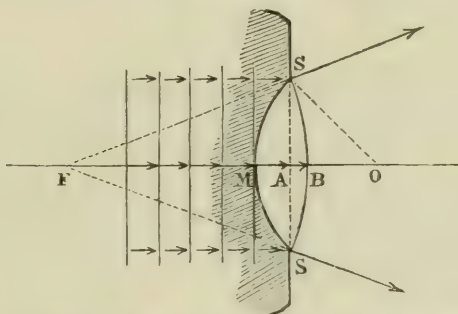
$$\mathcal{F} = \mathcal{R}(1-h). \quad \dots \dots (4) \text{ bis}$$

The formula, therefore, is the same for entrant plane waves whether the surface be convex or concave, the sign of \mathcal{F} following the sign of \mathcal{R} .

Case (ii.) *a. Emergent Wave; Surface Convex toward light*
(i. e. concave toward air into which wave emerges).

The plane wave would have had its front at S A S (fig. 7)

Fig. 7.



at a certain instant had its path lain wholly in the retarding medium; but the central portion being accelerated by its emergence at M into air reaches B, where B M is to A M as 1 to h . Hence the curve S B S, whose curvature is measured by the sagitta A B, is the impressed focal curvature. It will be noted that this is of opposite sign to that of the surface of emergence. Hence the sagitta A B must be taken with minus sign.

$$\begin{aligned} -AB &= BM - AM \\ &= \frac{1}{h}AM - AM \end{aligned}$$

$$+AB = AM \left(1 - \frac{1}{h}\right)$$

$$\mathcal{F} = \mathcal{R} \left(\frac{h-1}{h} \right) \dots \dots (6)$$

As before, for any two media having respective velocity-constants h_1 and h_2 , the formula becomes

$$\mathcal{F} = \mathcal{R} \frac{h_1 - h_2}{h_1}, \quad . \quad . \quad . \quad . \quad . \quad (5) \text{ bis.}$$

which, in the present case where $h_1 < h_2$, will give \mathcal{F} of opposite sign to \mathcal{R} .

Case (ii.) *b*. *Emergent Wave; Surface Concave toward light* (*i. e.* convex toward air into which wave emerges).

This is similar to the preceding, and yields the same formula.

Comparison of Case (i.)a with Case (ii.)b.

Comparing formula (4) with formula (6), we get for the two primary focal curvatures impressed respectively on plane waves passing in opposite directions through the curved surface,

$$\frac{\mathcal{F}_1}{\mathcal{F}_2} = -\frac{h_2}{h_1}; \quad . \quad . \quad . \quad . \quad . \quad (7)$$

whence, for the two primary focal lengths,

$$\frac{f_1}{f_2} = -\frac{h_1}{h_2}.$$

The focal lengths are, as the negative sign indicates, to be measured in opposite directions with respect to the surface. Also, taking the algebraic sum of the two primary focal lengths, we get

$$\begin{aligned} f_1 &= r \frac{h_1}{h_1 - h_2}, \\ f_2 &= r \frac{h_2}{h_2 - h_1} = r \frac{-h_2}{h_1 - h_2}, \\ f_1 + f_2 &= r \frac{h_1 - h_2}{h_1 - h_2} = r : \end{aligned}$$

and, as one of the focal lengths is negative, it follows that the difference of their lengths is equal to the radius of the curved surface.

C. CURVED SURFACE : CURVED WAVE.

The cases in which a wave possessing initial curvature passes through a curved surface and acquires a resultant curvature may be dealt with, apart from any further geometrical constructions, by applying the principle of super-

position of curvatures. Thus, take the case of a wave possessing initial curvature \mathcal{U} entering from air into a medium having velocity-constant h , and so curved that the focal power of the curved surface is \mathcal{F} . Then, as the wave enters the surface of the medium two effects will occur: its initial curvature will be altered in the ratio of the velocities, and there will be superposed upon it the focal curvature of the surface; or, in symbols,

$$\mathcal{V}_1 = h\mathcal{U} + \mathcal{F}_1. \quad . \quad . \quad . \quad . \quad . \quad (8)$$

For an emergent wave, possessing initial curvature \mathcal{U} in the medium, the formula will be

$$\mathcal{V}_2 = \frac{1}{h}\mathcal{U} + \mathcal{F}_2. \quad . \quad . \quad . \quad . \quad . \quad (9)$$

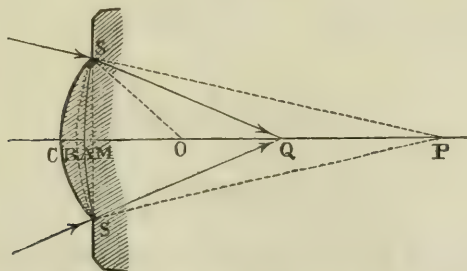
Or, for the case of a wave passing from a medium of velocity-constant h_1 to another of velocity-constant h_2 , the formula will be

$$\mathcal{V} = \frac{h_2}{h_1}\mathcal{U} + \mathcal{F}. \quad . \quad . \quad . \quad . \quad . \quad (10)$$

It is easy, however, to prove any one of the several cases that may arise, without in this way relying upon the principle of superposition. Take the case of a positive wave entering a positively curved surface.

Let SCS (fig. 8) be the surface of the medium, its cur-

Fig. 8.



vature being measured by the sagitta CM. There will be a certain moment when the entrant wave, converging toward P, would have had as its front SAS had its path lain wholly in air. But the central portion has entered the retarding medium at C, hence will only have advanced as far as B instead of A; B being such that $BC = h \cdot AC$. Hence the resultant wave will have the form SBS, and the sagitta of the resultant curvature is BM.

$$\begin{aligned}
 \text{BM} &= \text{CM} - \text{CB}, \\
 &= \text{CM} - h \cdot \text{AC}, \\
 &= \text{CM} - h(\text{CM} - \text{AM}), \\
 &= \text{CM}(1-h) - h \cdot \text{AM}; \\
 \mathcal{V} &= \mathcal{R} - h\mathcal{R} + h\mathcal{U}.
 \end{aligned}$$

But

$$\mathcal{R}(1-h) = \mathcal{S},$$

whence

$$\mathcal{V} = \mathcal{S} + h\mathcal{U}. \quad . \quad . \quad . \quad . \quad . \quad (8) \text{ bis.}$$

D. LENS FORMULÆ.

Case i. *Thin Lens; Plane Wave.*

In the case of a lens, the curvature \mathcal{S}_1 imprinted on a plane wave by entrance at the first surface may be regarded as an initial curvature of the wave which emerges through the second surface. Emergence into air will, as shown above, alter the curvature by augmenting it in the ratio of 1 to h , and superpose upon it the focal curvature \mathcal{S}_2 due to the second surface. Hence the whole resultant curvature \mathcal{S} imprinted by a *thin* lens on the plane wave will be

$$\mathcal{S} = \frac{1}{h} \mathcal{S}_1 + \mathcal{S}_2.$$

But

$$\mathcal{S}_1 = \mathcal{R}_1(1-h),$$

and

$$\mathcal{S}_2 = -\mathcal{R}_2 \left(\frac{1-h}{h} \right);$$

whence

$$\mathcal{S} = \mathcal{R}_1 \frac{1-h}{h} - \mathcal{R}_2 \frac{1-h}{h},$$

or

$$\mathcal{S} = (\mathcal{R}_1 - \mathcal{R}_2) \frac{1-h}{h}. \quad . \quad . \quad . \quad . \quad (11)$$

This formula may be compared with that in the current notation,

$$\frac{1}{f} = \left\{ \frac{1}{r_1} - \frac{1}{r_2} \right\} (\mu - 1).$$

In the case of a lens composed of a medium h_2 , lying between two other media h_1 and h_3 , the formula becomes

$$\mathcal{S} = \frac{1}{h_1 h_2} \{ \mathcal{R}_1 (h_1 - h_2) h_2 + \mathcal{R}_2 (h_2 - h_3) h_1 \}. \quad . \quad (12)$$

Case ii. *Thick Lens ; Plane Wave.*

If the lens has thickness d , the rule for expansion of curvature at end of § 4 above at once gives

$$\mathcal{K} = \mathcal{K}_2 + \frac{1}{h} \mathcal{K}_1 \frac{1}{1 \pm \mathcal{K}_1 d}, \quad (13)$$

or

$$\mathcal{K} = \left\{ \mathcal{K}_1 \frac{1}{1 \pm \mathcal{K}_1 (1-h)d} - \mathcal{K}_2 \right\} \frac{1-h}{h}. \quad . . (14)$$

Case iii. *Any Lens ; Any Wave.*

The principle of superposition at once gives the universal formula for all lenses bounded by identical media on the two sides :—

$$\mathcal{V} = \mathcal{U} + \mathcal{K}; \quad (15)$$

or, in words, the resultant curvature is the algebraic sum of the initial curvature and the impressed curvature. This may again be compared with the formula in current notation:

$$\frac{1}{v} = \frac{1}{f} - \frac{1}{u}.$$

The difference in sign attributed to the term $\frac{1}{u}$ arises from conventions adopted in the two systems.

E. TWO THIN LENSES AT DISTANCE APART.

The principle of expansion of curvature at once gives us as the equivalent focal power,

$$\mathcal{K} = \mathcal{K}_2 + \mathcal{K}_1 \frac{1}{1 + \mathcal{K}_1 d}, \quad (16)$$

where \mathcal{K}_1 and \mathcal{K}_2 are the focal powers of the first and second lenses, and d the distance between them. \mathcal{K} will be in *dioptries* if \mathcal{K}_1 and \mathcal{K}_2 are in *dioptries* and d in metric units. If the two thin lenses are close together, the resultant power is simply the algebraic sum of the powers of the separate lenses. One simply adds the *dioptries* of the separate lenses to find the resultant *dioptries*.

6. *Reflexion Formulæ.*

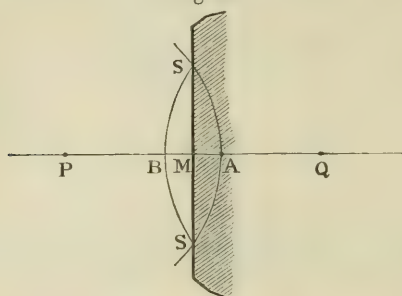
MIRRORS.

Case i. *Plane Mirror ; Curved Wave.*

The mirror (fig. 9) has surface S M S. The incident wave would have had front S A S at a certain instant had its path lain

wholly in air. The central portion of the wave, which would have reached A, travels backwards to B, an equal distance, in

Fig. 9.



the same time. The sagitta BM of the resultant curvature is equal to and of opposite sign to the sagitta AM of the initial curvature ; or

$$\mathcal{V} = -\mathcal{U}. \quad . \quad . \quad . \quad . \quad . \quad (16)$$

Case ii. *Curved Mirror; Plane Wave.*

There are two cases, equally simple, of convex and concave mirrors. One will suffice. Consider (fig. 10) a plane wave which at a certain instant would have arrived at SAS had its path lain wholly in air. The central portion of the wave has, however, struck at M, and marches backwards to B in same time as it would have taken to reach A. Hence

$$BM = AM,$$

or

$$BA = 2AM.$$

But AM measures the curvature of the mirror, whilst BA measures the curvature impressed on the plane wave. Hence

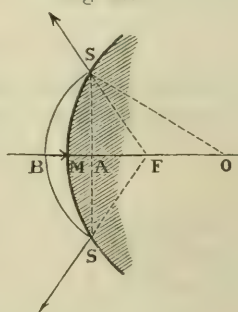
$$\mathcal{F} = 2\mathcal{H}. \quad . \quad . \quad . \quad . \quad . \quad (17)$$

Case iii. *Any Mirror; Any Wave.*

The principle of superposition at once leads to a general formula, expressing the sum of the two actions of the mirror on the wave ; it reverses its initial curvature, and then imprints a focal curvature upon it. In symbols,

$$\mathcal{V} = -\mathcal{U} + \mathcal{F}. \quad . \quad . \quad . \quad . \quad . \quad (18)$$

Fig. 10.



7. *Formula of the Lenticular Mirror.*

The lenticular mirror consists of a lens one face of which is silvered, so that the light passes twice through the lens, and suffers an intermediate reflexion at the silvered surface. If the focal power of the lens be \mathcal{F}_1 , and the curvature of the mirror-surface be \mathcal{R} , then the total curvature which the system impresses upon a plane wave will be equal to $2\mathcal{F}_1 + 2\mathcal{R}$, and there will be a reversal of the direction of the light, so that

$$\mathcal{F} = -2(\mathcal{F}_1 + \mathcal{R}). \quad . \quad . \quad . \quad . \quad . \quad (19)$$

8. *A Dioptrie Spherometer.*

In order the better to bring to practical issues the method of treating lens problems by curvatures, the author designed a special spherometer. In this instrument the readings of the scale are caused to indicate directly the dioptries of the curvature; that is to say, one whole turn of the micrometric screw corresponds accurately to a curvature having one metre as its radius. In the particular instrument now exhibited the pitch of the micrometric screw is 1 millimetre. To make this correspond to a curvature of one dioptrie, the radius of the contact-circle on which the three feet stand must be 44.71 millimetres. For, by reference to fig. 1, it will be seen that the radius PM of the contact-circle is such that

$$(PM)^2 = AM \cdot MB = 2rs - s^2,$$

where s is the sagitta of the curve. Hence, if $r=1000$ millimetres, and $s=1$ millimetre, $PM = \sqrt{1999} = 44.7102$. The screw itself is cut on a steel tube 8 millimetres in diameter. The tips of the three feet and of the central point are of phosphor-bronze, which does not scratch glass. The arms carrying the three feet are girder-shaped, thin, deep; so as to be very firm vertically; lateral flexure is immaterial. The tips of the three feet are adjusted by filing to chisel-shaped edges, forming little tangents to the imaginary circle of contact. The divided circle is not, as ordinarily, cut upon a flat plate, but is cut upon a thin cylinder of aluminium-bronze affixed to the screw by a light spider with three arms. The whole instrument weighs only 78 grammes. It was constructed for the author by Messrs. Nalder Brothers.

The formula for the focal power of any lens (formula no. (11), p. 244) consists of two factors—one depending solely on the shape of the lens, the other upon its material.

The latter factor, $\frac{1-h}{h}$, is a mere numeric; whilst the

former, being the difference of two curvatures, is itself a curvature. If the curvature thus determined by shape solely is expressed in dioptries, then, on multiplying by the numeric which depends on the nature of the material, the resultant power of the lens will also be expressed directly in dioptries. The dioptrie spherometer facilitates this desirable end by its directness of reading, quite apart from any incidental advantages which may result from the details of construction.

XXXIV. *On the Mechanics of Luminosity.*

By E. WIEDEMANN.

[Continued from p. 163.]

Experimental Measurements.

17. **I**N order to show how many of the questions raised in the foregoing admit of an experimental solution I have made a number of measurements in various directions. The method of investigation is briefly the following :—

Since in all measurements the coefficients of emission are to be expressed in calorimetric measure, *i. e.* in gramme-calories per second, the numbers given by the amyl-acetate lamp, serving as the source of light, for comparison at a given distance from the photometer-slit must be reduced to absolute intensity. This was done by comparison with the radiation of a glowing platinum wire. From these measurements we obtain at once the energy in gramme-calories per second radiated by 1 gr. platinum, and, consequently, relationships between the total energy and that radiated in a definite region of the spectrum, *e. g.* in the yellow. Next, the total brightness of gas-flame coloured with sodium was compared with the brightness of the amyl-acetate lamp in the yellow; and hence the coefficient of emission was calculated in gramme-calories per second for one gramme and for a molecule of sodium.

Then follows the determination of the quantity b , and from its value that of the store of luminous energy L , and therefore of the kinetic energy of the luminous motions.

Dependence of the Radiation of a Platinum Wire upon its Temperature.

18. The measurements were carried out in the first place for the D-line. The amyl-acetate lamp was always employed as the comparison-lamp.

The arrangement followed in these measurements and for

the determination of the brightness of the comparison-lamp in absolute measure was the following:—At a definite distance from the slit in the prolongation of the axis of the collimator of a Glan's photometer a platinum wire was placed, with its axis of length parallel to the slit, which was then ignited by means of a galvanic current. The source of light was placed before the comparison-prism.

The platinum wire d must be placed in a vacuum, since otherwise its loss of energy would depend not only—nor even chiefly—upon the radiation, but very largely also upon convection-currents. Since the wire expands when heated, it must not be clamped tight or it will become curved, and would thus not remain parallel to the slit. It was therefore fastened in the following manner (Pl. III. fig. 1). Its upper end was soldered to a strong brass wire m , at its lower end a long thick copper rod k was fastened, provided below with a point p . Above and below, the outer portions of two ground pieces σ and σ' were melted on to a cylindrical glass tube c of 38 millim. diameter. A brass wire m was cemented into the inner portion s belonging to the upper ground portion. The inner portion s' of the lower ground portion contained mercury q , which was put into connexion with the source of electricity by means of the copper wire r cemented in, into which the point p of the copper rod k plunged. When the platinum wire was heated by means of a current, the copper wire was stretched by the copper rod so that it remained perfectly straight. A side tube h connected the cylindrical tube with Töpler's mercury pump with Sundell's modifications and improvements*. It was exhausted as completely as possible. In a discharge-tube melted on at the same time and provided with a plate and a point 6 centim. from each other, discharges were scarcely to be obtained, using an induction-coil of moderate size. In fact the further action of the pump did not diminish the brightness of the wire. The heating of the glass wall by radiation was without influence, since, as far as the measurements of brightness and the electrical measurements were concerned, it was a matter of indifference whether the wire was maintained at the high temperature a longer or a shorter time.

The double-image prism in the photometer was so placed that the squares of the cotangents of the angles read were proportional to the brightness J of the platinum wire. The apparatus furnished to me differed from that described by Herr Glan in having the Wollaston's prism turned through an angle of 180° .

* *Beibl.* ix. p. 756 (1885).

The quantity of energy E lost per second by radiation during the constant luminosity of the platinum wire is proportional to the product of the resistance w of the wire, and the square of the intensity of the current i , or, if they are measured in ohms and amperes,

$$E = 0.24 wi^2,$$

where 0.24 is the necessary factor of reduction to convert energies measured in ohms and amperes into gramme-calories per second*.

In order to determine these quantities a Wheatstone's bridge (fig. 2) was constructed in the following manner†:—

The branch 1 contained a vertical rectangular bar of wood, α , surrounded by oil, upon which various constant known resistances were formed, as well as the reflecting galvanometer g_1 , serving to measure the strength of the current with the strong currents which had to be used here. A thick copper ring was employed instead of a multiplier, the resistance of which and its connexions was κ .

The branch 2 was formed by the platinum wire d to be investigated, whose resistance at temperature t may be called w_t .

The branch 3 contained a large resistance of y nominal Siemens units, and branch 4 a box of resistance-coils of from 1 to 5000 Siemens units, the accuracy of which had been previously tested.

The heating-current entered at the point 1, 3, and was furnished by six large Bunsen cells arranged three in series (the reason for not employing a Gramme or dynamo is given further on), and was led off at the point 2, 4. A variable resistance included in the circuit at W permitted the variation of the strength of the current, and with it the heat produced, and consequently the luminosity.

The resistance of the branches 3 and 4 was always so great in comparison with 1 and 2 that the variations of the current caused no perceptible variation in the intensity of the current in 1 and 2. (The resistance of 1 and 2 never exceeded 2 Siemens units, and the resistance in 3 and 4 was not less than 400 Siemens units.)

The sensitive galvanometer g_2 serving for the measurement of the resistances was inserted between the points 1, 2 and 3, 4. By changing the plugs in the box of resistance-coils in branch 4 the resistance was altered until the galvanometer remained at rest on opening and closing the key.

* Kohlrausch, *Leitfaden d. pract. Physik*. 6th ed. p. 334 (1887).

† Compare Bottomley, *Phil. Trans. Roy. Soc. London*, cxviii. p. 429 (1887).

If the resistances in 1, 2, 3, 4 are w_1, w_2, w_3, w_4 , then $w_1 : w_2 = w_3 : w_4$. But since w_1, w_3, w_4 are known, we have at once w_2 , the resistance of the platinum wire under investigation. After the conclusion of each series of experiments the constant of the galvanometer g_1 was determined by means of a voltmeter.

The resistance of the platinum wire at the ordinary temperature of 15° was determined by means of an ordinary Wheatstone bridge, with one Leclanché cell to furnish the current, so as to avoid heating. The resistances a, m , and g were found in the same way.

19. The first series of experiments had for its object to determine how the intensity of light in the yellow increased as the total radiation increased upon rise of temperature. The region of the spectrum examined was close to the sodium line. The platinum wire was at a distance of 45 millim. from the slit. The amyl-acetate lamp was at a distance of 20 centim. Then for various degrees of luminosity we obtain the resistances w_t , their ratio to the resistance w_{15} at 15° C. $\frac{w}{w_{15}}$, the intensities of the current i and the quantities $w_i i^2$ proportional to the total radiation (assuming that the wire has the same temperature at all points of its section), and, lastly, the brightness J in the yellow.

$w_{15} = 0.2555$				
$w_t = 0.6314$	0.6609	0.6923	0.7274	0.7736
$i = 1.997$	2.195	2.478	2.843	3.383
$\frac{w_t}{w_{15}} = 2.471$	2.587	2.709	2.847	3.028
$w_i i^2 = 2.518$	3.184	4.251	5.879	8.854
$J = 0.025$	0.0935	0.189	0.455	1.761

According to the data of Sir William Siemens*, the ratio $w_t/w_{15} = 2.471$ would correspond to about 700° C., and that of 3.028 to about 1000° C.; with which the observation agrees that in my experiments the brightness increased seventy times; whilst, according to M. Violle†, it increased for the portion of the spectrum corresponding to the D-line from 0.05 to 3.6 between 715° C. and 1045° C., that is, about seventy-two times.

The above values show that the radiation in the visible spectrum rises much more rapidly than the total radiation. The former (J) increases 70 times, the latter (E , measured by means of $w_i i^2$) $3\frac{1}{2}$ times.

* Proc. Roy. Soc. Lond. xxxv. p. 166 (1883).

† *Comptes Rendus*, xcii. pp. 866 & 1204 (1881).

Within the visible spectrum the like is shown by older experiments. Thus, M. Violle finds that between 954° and 1775° the intensity for the C-line increases 154 times, for the D-line 219 times, and for the E-line 307 times. The intensity therefore increases the more slowly the less refrangible the rays are. Moreover the total radiation contains the infra-red rays, whose increase will be much slower than is shown by the above numbers. At the same time we see that even at the highest temperature the radiation in the infra-red furnishes an overwhelming contribution to the total radiation. For the extraordinarily rapid increase of the radiation in the visible part of the spectrum is not sufficient, as the numbers given above show, to compensate the very much slower increase in the infra-red.

Herr H. Schneebeli* has obtained the same results in experiments with Swan lamps. Whilst the total radiation increased in the proportion of 1 to 4, the optical radiation increased in the proportion of 0.3 to 24. The Swan lamps are, however, scarcely so highly exhausted but that one must suppose that a portion of the loss of energy is due to convection. Further H. Schneebeli employed a gramme-machine to furnish the current, of which the current-strength varied between certain maximum and minimum values. The quantity of energy given off in the unit time is then, if we denote by J the mean current-strength as shown by the galvanometer, and by i that existing at any time t , and by w the resistance regarded as constant, not wJ^2 , but $\int_0^1 wi^2 dt$. It is not possible to decide between these two values without knowing what the action of the Gramme-machine was.

When Hr. E. Lecher† nevertheless, in a research on the visible spectrum, is able to make the brightness equal in all parts of the spectra yielded by two different platinum wires by weakening the whole of the brighter of the two spectra, the reason no doubt is that, as the above-mentioned experiments of M. Violle show, the changes in the visible spectrum for the different colours are not remarkably different. Moreover, for the small brightnesses in question, only quite limited regions of the spectrum contribute to the physiological light-sensation‡.

* Wied. *Ann.* xxii. p. 433 (1882). † *Ibid.* xvii. p. 512 (1882).

‡ I wish to avail myself of this opportunity to make a remark on the construction of formulæ which represent the emission as a function of the temperature and the wave-length. With the exception of the formula of Herr Stefan, according to which the radiation is proportional to the fourth power of the absolute temperature, all have more or less the character of interpolation-formulæ.

If a formula, based upon theoretical considerations, is really to represent

Total Radiation from one Square Centimetre and one Gramme Platinum.

20. In the determination made for the purpose of evaluating the amyl-acetate lamp in absolute measure, the distance between the slit and the wire was 44 millim. This is made up of the distance of the slit from the edge of the glass tube and the radius of the tube. The distance of the amyl-acetate lamp from the slit was 216 millim. The length of the wire was 7.6, its thickness 0.26 millim. Consequently the radiating surface $O = 2\pi rl = 0.6205$ square centim.

If the current-strength (in amperes) is i , the resistance for current-strength equal to w_1 (in ohms Ω), the resistance at 15° w_{15} , the energy radiated by the whole wire $F = 0.24 wi^2$ (in cm. g. sec. cal.), the readings α on the photometer for the part of the spectrum close to the sodium-line, the brightness J measured by $\cotan^2 \alpha$, the energy radiated by the unit surface $E = G \div O$, then the following (amongst other) data were obtained:—

$$\begin{aligned} w_{15} &= 0.239 \Omega, & w_i &= 0.757 \Omega, & w_i/w_{15} &= 3.17; \\ i &= 4.00, & F &= 2.91, & E &= 4.68; \\ \alpha &= 36^\circ 30', & J &= \cotan^2 \alpha = 1.827. \end{aligned}$$

A square centimetre of the surface of a platinum wire heated to about 1000° C. consequently radiates about 4.7 gr. calories in a second.

The value of E agrees in order of magnitude with that of H. Bottomley* for the highest temperature used by him; mine is higher. This is explained by the fact that the temperature used by me is higher than his†.

the above-mentioned dependence over a considerable region, then in the first place no luminescence-phenomena must occur with the body in question, and in the second place the molecules and atoms which with their æther envelopes build up the body must undergo no changes. But such a change certainly takes place in all bodies which show a great change in specific heat upon increase of temperature. Since the specific heats in the liquid and gaseous and in the liquid and solid conditions exhibit like coefficients of change, so far at least as present investigations extend, the quantity of heat corresponding to the change in specific heat can only result from intramolecular work. But anything that brings about an increased porosity of the molecular condition, or an increased intramolecular activity, must modify the emission and absorption of light in a way not immediately determinable.

* Phil. Trans. cxviii. p. 429 (1887).

† It is only after completion of the present investigation that I have become acquainted with the work of Messrs. O. Tumlirz and A. Krug, "On the Energy of the Radiation of Heat at a White Heat" (*Sitzungsber. d. Wien. Akad.* xcvi. pp. 1521-1529, 13 12, 1880), which follows a previous paper by one of the authors (*Wien. Ber.* xcvi. p. 1007, 1883); as

The number 4.7 obtained for the total energy is taken as the basis of all the following calculations.

21. From these numbers certain further conclusions can be obtained.

We will first calculate the quantity of energy radiated per second from 1 gramme and from one atom of platinum.

The above quantity of energy, 4.7 calories, is emitted by a layer of unit surface, which has such a thickness that the rays coming from the deepest points still perceptibly issue from the surface. The different layers contribute in different degrees to the radiation. For the sake of simplicity we will assume that all the layers do this equally. If d is the thickness of the radiating layer, s the specific gravity of the radiating substance, then the weight of the radiating layer under the unit surface is $\gamma = sd$. For d I put the approximate value $d = 10^{-5}$ cm., corresponding to the numbers of Herr Wien*. Further, for platinum, $s = 21.5$; then the weight of the radiating layer for platinum is 2.15×10^{-4} gr. One gramme of platinum consequently emits in one second a quantity of energy, 2.2×10^4 .

The total emission-coefficient of 1 gramme of platinum at the temperature used of about 1000° C. is therefore

$$S = 2.2 \times 10^4 \text{ gr. calories per second.}$$

But further, according to the calculations of van der Waals†, the absolute weight of a single atom of hydrogen is 7.5×10^{-23} gr., the atomic weight of platinum 194; consequently the weight of an atom of platinum is, in round numbers, 15×10^{-21} gr. Consequently an atom of platinum at the temperature of about 1000° C., assumed for the purposes of our calculations as that of the platinum wire, emits $2.2 \times 10^4 \times 15 \times 10^{-21} = 3.3 \times 10^{-16}$ gr. calories per second.

We will further determine the relationship existing between the quantity of heat emitted at 1000° C. and the quantity

well as that of Herr Tumlirz, "Calculation of the Mechanical Equivalent of Light from the experiments of H. J. Thomsen" (*Ibid.* pp. 1625-1632. 20 12. 1888). This gives the radiation of glowing platinum per square centimetre, and a comparison of the brightness of the glowing platinum with the acetate lamp. My values agree with his also as to order of magnitude. The differences may be explained by the fact that different wires behave differently; and that these gentlemen, whose object was the exact determination of this magnitude, have introduced a number of corrections which I did not consider necessary. As I was only concerned to determine the order of magnitude of the several quantities, nothing essential in the conclusions is altered by the introduction of the values of other observers instead of mine.

* Wied. *Ann.* xxxv. p. 57 (1888).

† Cf. R. Rühlmann, *Mech. Wärmetheorie*, ii. p. 247 (1885).

required to raise the quantity of platinum in question from 0° C. to 1000° .

The mean specific heat of platinum between 0° and t° , according to Violle*, is

$$c_0^t = 0.0317 + 0.056t;$$

and therefore between 0° and 1000° C.,

$$c_0^{1000} = 0.038.$$

To heat one gramme from 0° C. to 1000° C. we therefore require $1000 \times 0.038 = 38$ calories; and to heat the surface-layer,

$$2 \times 10^{-4} \times 38 \text{ cal.} = 7.6 \times 10^{-3} \text{ cal.}$$

The quantity of energy radiated per second at 1000° is therefore about 600 times greater than the quantity communicated in heating from 0° to 1000° .

If, further, we have a platinum wire of r centim. radius and 1 centim. length, then at 1000° the quantity of energy lost per second by radiation M , and the quantity W communicated in heating from 0° to 1000° , are given by the formulæ

$$M = 2\pi r \times 4.7, \quad W = \pi r^2 \times 21.5 \times 38;$$

therefore

$$W/M = 87 \times r.$$

We see from this that, with a wire about $\frac{1}{42}$ centim. in thickness, the energy radiated in a second and that communicated in heating from 0° are nearly equal. With thinner wires the latter diminishes very rapidly in comparison with the former.

Exactly similar considerations of course apply to the case of glowing and radiating platinum foil.

22. The method employed for the determination of the radiation gives it, in the first place, according to order of magnitude. The numbers just quoted show, in fact, how extraordinarily great the radiation is. The surface-layer must therefore cool rapidly. The loss of energy thus caused is instantly supplied by conduction from the interior hot portions at the expense of the work done by the current. Since the outer portions are, in any case, cooler than the inner, their resistance must be less. But the resistance measured is a mean of the various concentric layers. Therefore it is not at once possible, without a thorough inquiry into the relationships of conductivity for heat &c., to obtain a reliable conclusion, from the observed resistances, as to the actual temperature of the radiating surface†.

* *Comptes Rendus*, lxxxv. p. 543 (1877).

† Compare also (amongst others) the work of G. Basso, *Natura*, iii. pp. 225, 304.

The influence of these disturbing circumstances may be determined by heating a platinum wire to a definite temperature in an air-bath and measuring the intensity of the light emitted at a definite part of the spectrum, and at the same time determining its resistance. Then the wire is heated by means of a current to the same brightness and its resistance determined again. From the difference in resistance observed in the two cases account can be taken of the complications in question. The experiments should be made with wires of different thickness.

Comparison of the Amyl-acetate Lamp with Glowing Platinum.

23. From these determinations we will turn to the definite evaluation of the amyl-acetate lamp in absolute measure. Care must be taken that the platinum wire is linear, but the amyl-acetate flame, on the other hand, flat; *i. e.* so that the rays from the former traversing the slit only fill a portion of the objective, whilst those from the latterly entirely fill it so soon as the flame is sufficiently near to the slit, as is the case in our experiments.

How account is to be taken of these circumstances in their influence on the brightness is explained in the following.

(a) First, we calculate the quantity of energy reaching unit length of the slit from the platinum wire.

Let h be the diameter of the diaphragm in the collimator which limits the pencil of rays issuing from it, e its distance from the slit, η the distance of the wire from the slit, and δ the thickness of the wire. The pencil of rays drawn from the diaphragm through a point of the slit intersects on the surface of the wire an area which, projected on the meridian-plane at right angles to the axis of the collimator, has a breadth δ and a height γ , as calculated from the proportion

$$\gamma : \eta = h : e, \quad \gamma = \frac{\eta h}{e}.$$

The quantity h/e occurring here can be calculated as follows.

At a distance a of 35 centim. from the slit a scale was placed at right angles to the axis of the collimator and to the length of the slit, and a light was moved along the scale until an observer at the telescope announced that the light could no longer be seen. On moving the eye sideways right or left the distance of these two points was 2.2 centim. : then

$$h/e = l/a = \frac{2.2}{35.0} = 0.063.$$

Let us take, as a first approximation, the law of cosines* as holding good for the radiation, then we replace the semi-cylindric surface of the wire radiating to each point of the slit by the rectangle $\gamma\delta=f$.

Let the quantity of energy radiated by each square centimetre be E , then the surface f gives, on the whole, the quantity of energy Ef .

If the width of the slit is s , then the unit of length of the slit receives a portion which is to the total radiation as the surface of the slit corresponding to the unit of length s . 1 is to the half surface of a sphere of radius η . (In the quantity E determined by experiment we have only the quantity of energy radiated outwards, and not that radiated towards the interior of the wire.) This fraction is $s/2\pi\eta^2$.

Hence upon the unit length of the slit there is radiated from the surface f of the glowing platinum wire a quantity of energy

$$A = \frac{s}{2\pi\eta^2} Ef = \frac{\eta h}{e} \delta \frac{s}{2\pi\eta^2} E = \left(\frac{h}{e}\right) \frac{\delta \cdot s}{2\pi\eta} E.$$

In our experiments

$$h/e = 0.063, \quad \delta = 0.026 \text{ cm.}, \quad \eta = 4.4 \text{ cm.};$$

consequently

$$A = 0.063 \frac{0.026}{2\pi \times 4.4} sE = 0.046sE.$$

(b) We will now calculate the similar expression for the energy sent to the slit by a flat-shaped source of light of considerable extent, like that of the amyl-acetate lamp, or the Bunsen flame coloured with sodium, which is so near to the slit that the cone passing through the diaphragm of the collimator and a point of the slit in its prolongation towards the flame is completely filled with luminous particles.

The cone from the diaphragm through a point of the slit cuts the flame in a circle; if the flame is at a distance η' from the slit, and if δ' is the diameter of this circle, then the

* The validity of the law of cosines may, upon theoretical grounds, be open to doubt. As is well known, it is established by regarding as the radiating quantity that contained in a parallelopiped whose base is the radiating surface, and of which the edge forms a portion of the prolongation of the rays under investigation, equal to the depth from which, in general, rays still issue. But it is certainly not the particles contained in this parallelopiped which give the rays issuing in the direction in question, since in their introduction the refraction from metal into air is neglected, of the existence of which (even before the direct proof given by Herr A. Kundt) evidence was offered by the strong polarization of the emergent light. Further experimental investigations are required to explain the contradiction between theory and the observations of Herr Möller (*Wied. Ann.* xxvi. p. 266), which tend to confirm the law of the cosine.

radiating surface is

$$\frac{\pi}{4} \delta'^2 = \frac{\pi}{4} \left(\frac{h}{e}\right)^2 \eta'^2.$$

If a square centimetre emits a quantity of energy E' , then our surface yields a quantity of energy,

$$\frac{\pi}{4} \left(\frac{h}{e}\right)^2 \eta'^2 E'.$$

Of this the fraction which reaches the unit length of the slit is $s/4\pi\eta'^2$. We must here divide by the whole surface of the cone, since the sodium flame is transparent to its own rays. The quantity of energy actually falling upon the slit is therefore

$$A' = \frac{s}{4\pi\eta'^2} \cdot \frac{\pi}{4} \left(\frac{h}{e}\right)^2 \eta'^2 E' = \frac{s}{16} \left(\frac{h}{e}\right)^2 E' = 0.0325 s E'.$$

The distance η' thus does not occur in the final result, since the radiating surfaces increase as the squares of the distances. We may say that the quantity A' is the fraction of the total energy which passes through the diaphragm. Strictly speaking, account should also be taken of the circumstance that the flame represents not a space bounded by two parallel very large surfaces, but a cylinder. Nevertheless, what we thus neglect is small in comparison with the other sources of error. We have further neglected the fact that the slit is not a portion of the sphere, but occupies a tangent plane.

(c) We therefore obtain for the ratio of the energies which reach the slit from an extended source of light, and a narrow linear source

$$\frac{A'}{A} = \frac{1}{16} \left(\frac{h}{e}\right)^2 \frac{2\pi\eta E'}{\delta E}, \text{ or } \frac{E'}{E} = 16 \left(\frac{e}{h}\right)^2 \frac{\delta}{2\pi\eta} \frac{A'}{A}.$$

With the dimensions of our apparatus in particular

$$\frac{E'}{E} = 0.24 \frac{A'}{A}, \text{ or } E' = 0.24 \frac{A'}{A} E.$$

The ratio of the energy of a source of light with a continuous spectrum, and that of the platinum wire at a definite point of the spectrum is obtained at once from the readings of the photometer. We have seen above that the brightness of the platinum is 1.827 times greater than that of the amyliacetate lamp for the yellow in the neighbourhood of the D line. Hence

$$A/A' = 1.827,$$

$$A'/A = 0.547,$$

and we obtain for the energy of unit surface of the amyli-

acetate lamp expressed in terms of that of the glowing platinum for the yellow

$$E' = 0.24 \cdot 0.547 E = 0.13 E.$$

Comparison of Sodium Flame and Glowing Platinum.

24. After this determination we may further compare the brightness of the amyl acetate lamp for the yellow with that of a gas-flame coloured yellow by sodium, according to the method of Herr Ebert*, and thus the latter also with the brightness of the yellow of glowing platinum.

If, then, we wish to determine the ratio of the radiation of the sodium flame corresponding only to the yellow sodium lines and the total energy of radiation of the glowing platinum, we must first determine the ratio of the latter to the radiation which reaches a definite portion of the yellow.

For this purpose we will make use of the results of Mouton†, by assuming, without doubt correctly, that the temperature of the platinum wire in our absolute measurements is nearly equal to that of the platinum wire in Mouton's Bourbouze lamp.

If this is not exactly the case, and consequently the final value is not quite accurate, yet its order of magnitude can in no case be affected.

In order to obtain a part of the radiated energy which belongs to a definite portion of the spectrum situated in the neighbourhood of the D-line, the following method was adopted. A curve was drawn upon paper according to Mouton's numbers, which represented the distribution of energy as a function of the wave-length. The wave-lengths were measured in 1μ , the energies in any convenient unit. By division of the weight g of a piece of the curve-paper of known area by the weight G of the area included between the curve and the axis of abscissæ, we obtain for the fraction of the total energy corresponding to unit area

$$a = \frac{g}{G}.$$

* Wied. Ann. xxxii. p. 345 (1887).

† *Compt. Rend.* lxxxix. p. 295, 1879; *Beibl.* iii. p. 868, 1879. The following calculation of course proceeds upon the assumption that we obtain the whole quantity of radiated energy in the bolometer or the thermopile, or that the substance of the bolometer absorbs even the extreme infra-red rays. This may be tested experimentally by comparing the curve of energy determined by the bolometer with the total expenditure of energy as measured by resistance and intensity. I should have liked to have determined the distribution of energy for the wire employed by me, but unfortunately this was not possible with the very unfavourable conditions of the Erlanger Institute—it is so exceptionally damp that it is not possible to set up rock-salt prisms &c. for the purposes of an extended research.

In our case

$$a = 0.083.$$

If we make the slit so wide that when illuminated by homogeneous light of wave-length λ it has a breadth in the spectrum corresponding to a difference of wave-length Δ at this place, and if we now illuminate it with white light, then every point at the same place receives rays between the wave-lengths λ and $\lambda + \Delta$.

If the ordinate corresponding to the wave-length λ in the energy-curve is y , and that corresponding to $\lambda + \Delta$ is y_1 , then, since Δ is always small, the area included by the ordinates y and y_1 , the curve, and the axis of abscissa is

$$F = \frac{y + y_1}{2} \Delta,$$

and the corresponding energy is

$$a \frac{y + y_1}{2} \Delta.$$

The breadth Δ of the slit illuminated by the sodium flame in our experiments amounted to 0.22 of the distance between the sodium and lithium lines in the spectrum; the wave-length of the sodium line is 0.59, of the lithium line 0.67. Each point of the spectral image receives then rays between the wave-lengths $\lambda = 0.59$ and $\lambda + \Delta = 0.59 + (0.67 - 0.59) \cdot 0.22 = 0.6076$. Further, to the abscissæ 0.59 and 0.6076 correspond the ordinates $y = 11.35$ and $y_1 = 13.33$; the above-mentioned surface is therefore

$$F = \frac{11.35 + 13.33}{2} \times 0.0176 = \frac{24.68}{2} \times 0.0176.$$

But to this surface there corresponds a fraction ξ of the total energy

$$\xi = 0.083 \times \frac{24.68}{2} \times 0.0176 = 0.00180 = \frac{1}{556}.$$

Having thus determined the energy corresponding to this definite breadth of slit from measurements with our apparatus, we find for the sodium flame the whole, but for the platinum wire only the $\frac{1}{556}$ of the total radiated energy.

A' and A are the measured brightnesses of the sodium flame and of the platinum wire, in reference to that of the amyl-acetate lamp; they are proportional to the squares of the cotangents of the readings on the photometer.

$$A' = \text{const.} \cotan^2 a', \quad A = \text{const.} \cotan^2 a,$$

where the constants have the same value.

Hence

$$\frac{A'}{A} = \frac{\cotan^2 a'}{\cotan^2 a},$$

therefore

$$E' = 0.24 \frac{\cotan^2 a'}{\cotan^2 a} \xi E.$$

In our case

$$a = 36^\circ 30', \quad E = 4.7, \quad \text{and} \quad \xi = \frac{1}{55.6},$$

so that

$$E' = 0.24 \frac{4.7 \cotan^2 a'}{55.6 \cotan^2 36\frac{1}{2}} = 0.00203 \frac{\cotan^2 a'}{\cotan^2 36\frac{1}{2}} \text{ cm. g. sec. cal.}$$

Coefficient of Total emission of 1 gr. sodium in absolute measure.

25. In order to test, in the first place, the dependence of the emission of light upon the quantity of sodium chloride contained in the unit volume, two solutions of sodium chloride were scattered into a flame in the mode described by Herr Ebert, exactly in the same way. They contained in 1 cub. centim. respectively

(a) $M = 0.0304$ gr. sodium, (b) $M_1 = 0.0132$ gr. sodium.

Their density was nearly unity.

A portion of the flame was placed opposite to the slit, which appeared uniform in the whole section. Its diameter is 2 cub. centim.

The readings a' on the photometer and the corresponding $\cotan^2 a'$ were for

$$(a) \ a' = 31^\circ, \quad \cotan^2 a' = 2.770,$$

$$(b) \ a'_1 = 42^\circ, \quad \cotan^2 a'_1 = 1.233.$$

Therefore, very nearly,

$$M : M_1 = \cotan^2 a' : \cotan^2 a'_1,$$

viz. :—

$$0.0304 : 0.0132 = 2.770 : 1.233.$$

The brightness therefore increases nearly proportionally to the quantity of salt present, which is also what Herr Gouy* has found.

For solutions of sodium carbonate, which contain in the unit volume the same quantities of sodium as the above sodium chloride solutions, the same brightnesses were found.

26. We will now calculate the quantity of sodium which in the first of these solutions yields the observed brightness, and the corresponding quantity of energy.

2100 cub. centim. of gas-mixture pass through the burner per minute†. The velocity at this point is, therefore,

* *Ann. de Chem. et de Phys.* [5] xviii. p. 5 (1879).

† (*cf.* also H. Ebert, *Wied. Ann.* xxxiv. p. 83 (1888).

$$\frac{2100}{3 \cdot 14 \times 1^2} = 670 \text{ centim.}$$

i. e. in each minute a column of gas 670 centim. long passes in front of the slit. In 30 minutes 1·025 gr. would be scattered, therefore in 1 minute 0·034 gr. A column of 1 centim. height and 2 centim. diameter contains therefore

$$\frac{0 \cdot 034}{670} = 5 \times 10^{-5} \text{ gr.,}$$

and in 1 cub. centim.

$$\frac{5 \times 10^{-5}}{3 \cdot 14} = 1 \cdot 59 \times 10^{-5} \text{ gr. fluid dust.}$$

With the concentration chosen 1 cub. centim. of the flame contains

$$4 \cdot 8 \times 10^{-7} \text{ gr. sodium.}$$

Let us now calculate the quantity of sodium in a parallelo-piped of unit height and breadth, therefore of the unit of radiating surface and of the thickness of the flame as depth, *i. e.* 2 cub. centim.; it contains in round numbers

$$9 \cdot 6 \times 10^{-7} \text{ gr. sodium.}$$

This quantity of $9 \cdot 6 \times 10^{-7}$ gr. sodium therefore radiates the quantity of energy per second

$$E' = 0 \cdot 00203 \frac{\text{contan}^2 31}{\text{contan}^2 36 \frac{1}{2}} = 0 \cdot 00308 \text{ cm. g. sec. calories.}$$

The coefficient of total emission of sodium, i. e. the quantity of energy radiated by 1 gr. sodium in the two yellow lines of the Bunsen flame amounts to

$$3210 \text{ g. calories per second,}$$

from which we obtain upon the assumption (no doubt not strictly correct) of equal brightness, 1600 gr. calories per second for each line.

An atom of sodium weighing $1 \cdot 7 \times 10^{-21}$ gr. emits per second

$$5 \cdot 5 \times 10^{-18} \text{ gr. calories.}$$

27. We found before that 1 gr. platinum radiates on the whole $2 \cdot 2 \times 10^4$ gr. calories per second, now we find that with sodium for the two isolated spectrum lines the same value amounts to $3 \cdot 2 \times 10^3$, which is not so much less. It is as if the energy emitted, which with platinum is distributed throughout the entire spectrum, were with sodium concentrated in the two lines.

In the case of sodium we have besides the energy of the infra-red rays present according to the researches of Ed.

Becquerel *, so that the coefficient of total emission of sodium for all rays together is greater than $3 \cdot 2 \times 10^3$.

True Coefficient of Emission of 1 gr. Sodium. Comparison with Platinum. Application of Kirchhoff's Law.

28. From the data for the coefficient of total emission for sodium, and the breadth of the sodium lines, we may obtain the true coefficient of emission for the unit breadth in the spectrum (*cf.* p. 161) for 1 gr. sodium.

For this purpose we have only to divide the energy emitted by the breadth of a sodium line; this according to diffraction experiments (see below 32, p. 265) is $\frac{1}{4}$ of the distance between the centres of the two sodium lines—that is $0 \cdot 15 \mu\mu$. The true coefficient of emission is then, if we take $1 \mu\mu$ as unit for the wave-length,

$$S_{Na} = \frac{1600}{0 \cdot 15} = 10700 ;$$

i. e. 1 gr. sodium in a region of the spectrum of the breadth $1 \mu\mu$ would emit per second 10700 g. cal., or in round numbers $1 \cdot 1 \times 10^4$ if the brightness throughout this region were alike.

29. We may further compare the true coefficient of emission of sodium vapour with that of solid platinum. We have seen above (p. 259) that since the ordinate of the curve of energy of platinum at the point which corresponds to the sodium line is 11·35, the energy radiated within the region of the spectrum of breadth $1 \mu\mu$ by platinum is

$$\xi E = 0 \cdot 0_283 \times 11 \cdot 35 \times 0 \cdot 001 \times E = 9 \cdot 4 \times 10^{-5} E.$$

Thus 1 gr. platinum emits (*cf.* p. 254) in this region

$$9 \cdot 4 \times 10^{-5} \times 2 \cdot 2 \times 10^4 = 2 \cdot 1 \text{ cal.}$$

The ratio of the true coefficient of emission of sodium S_{Na} and platinum S_{Pt} is therefore per gramme

$$\frac{S_{Na}}{S_{Pt}} = \frac{1 \cdot 1 \times 10^4}{2 \cdot 1} = 5 \times 10^3.$$

The coefficient for sodium is thus much greater than that for platinum.

30. A film of platinum of 1 sq. centim. area and 10^{-5} centim. thickness, which contains 2×10^{-4} gr. platinum is almost opaque.

According to Kirchhoff's law a film of sodium vapour which for equal area contains less substance in consequence of its greater coefficient of emission for its particular rays,

* Ed. Becquerel, *Compt. Rend.* xcix. p. 374 (1884).

must also be opaque ; *i. e.* a film which contains per square centimetre

$$\frac{2 \times 10^{-4}}{5 \times 10^3} = 4 \times 10^{-8} \text{ gr. Na.}$$

In the sodium flame examined by us there is present in a layer of 1 square centimetre 6.9×10^{-7} gr., that is about twenty times as much as would be necessary to produce such opacity with the platinum.

If, therefore, Kirchhoff's law is to hold good here the flame must be absolutely opaque for the yellow rays. In fact such a flame shows reversal ; *i. e.* in the centre of each of the yellow sodium lines a dark line appears when a ray of white light traverses it, nevertheless the absorption is by no means so great as one would have expected according to Kirchhoff's law, since the dark line is confined to the centre. Hence it would seem that in the sodium flame luminescence phenomena appear together with the usual luminous phenomena. In fact, highly complicated chemical processes occur in such a flame ; further researches will show this more clearly.

Direct Comparison of the Coefficients of Total and True Emission of Platinum and Sodium.

31. In continuation of the foregoing, an experimental arrangement was employed, which will find frequent application in later investigations.

In one and the same flame sodium was distributed and a platinum wire was heated to luminosity, and the brightness of each was compared with the amyl-acetate lamp.

The apparatus shown in fig. 3 (Pl. III.) was employed. In the interior of an Ebert's* burner, B, a thin platinum-wire 0.26 millim. thick was fastened at *a* ; it had thus the same thickness as the wire previously investigated, which was heated by the current ; at its upper end it was attached to a small hook which was suspended to one arm of a lever, movable about the horizontal axis *e*, which was capable of being adjusted as to height. The lever was weighted on the other side by the weight *f* in order to keep the wire stretched when hot.

Then water only was scattered in the flame, which was colourless, and the brightness of the platinum wire was measured ; then by a slight movement of the support the wire was put out of the field of view, the flame was fed with sodium solution, and its brightness measured again. These measurements showed that the ratio of brightness of the pla-

* Wied. Ann. xxxii. p. 345 (1887).

tinum wire to that of the sodium flame was almost the same as in the previous experiments in which the wire was heated by a current. There is no object in giving the particular values here.

In exactly the same way experiments were made with solution of strontium chloride, which yields a spectrum of bands, solutions equivalent to the sodium chloride solution being scattered in the flame. The result obtained was that the total brightness is of the same order as that of the sodium flame, which also may be inferred from the strong coloration of the flame.

Here, therefore, nearly the same total energy is distributed over a series of bands.

32. We will now determine the ratio of the true coefficients of emission of two sources of light. For this purpose let us consider the following points:—

If we have a narrow spectral line whose boundaries are at λ_1 and λ_2 , and if we examine this with a spectral-photometer of small dispersion, it appears in the spectrum of the same width as the slit. Let the dispersion be so chosen that the edges of the slit correspond to wave-lengths λ and $\lambda + \Delta$ in the spectrum. If then, by means of a spectrophotometer, we compare the brightness of the spectrum-line λ and of a continuous spectrum, then each point in the image of the slit, corresponding to the line-spectrum, receives rays between λ_1 and λ_2 , and in the image of the slit corresponding to the continuous spectrum each point receives rays between λ and $\lambda + \Delta$. If the intensities corresponding to the wave-length λ are in the two cases i' and i'' respectively, then the total intensities J_1 and J_2 in the first and second image respectively are:—

$$J_1 = \int_{\lambda_1}^{\lambda_2} i' d\lambda \text{ and } J_2 = \int_{\lambda}^{\lambda + \Delta} i'' d\lambda.$$

The quantities i' and i'' are proportional to the true coefficients of emission; the ratio of the intensities is therefore

$$V = \int_{\lambda_1}^{\lambda_2} i' d\lambda : \int_{\lambda}^{\lambda + \Delta} i'' d\lambda.$$

In the simplest case we may assume that i' and i'' are constants, *i. e.* that the spectral lines between λ_1 and λ_2 , as also the continuous spectrum between λ and $\lambda + \Delta$, possess everywhere the same brightness, then

$$V = \frac{i'}{i''} \cdot \frac{\lambda_2 - \lambda_1}{\Delta}.$$

The observed ratio of intensities is therefore proportional to

the ratio of the true coefficients of emission multiplied by $(\lambda_2 - \lambda_1)/\Delta$.

We may determine i'/i'' directly, if we observe with a spectrophotometer possessing so great a dispersion that the line also appears as a continuous band, for then the slit appears illuminated by homogeneous light of a width which is distinctly smaller than the spectral image of the line, it behaves therefore as a portion of a continuous spectrum; instead of $\lambda_2 - \lambda_1$ we have Δ , and that the more accurately the greater the dispersion, and the ratio of brightness measured in the apparatus is itself i'/i'' .

In order to determine the ratio i'/i'' for a sodium flame and the amyl-acetate lamp, the following arrangement was made:—The spectrum apparatus consisted of two telescopes belonging to Herr Ebert and a plane Rowland's grating. The collimator was provided with an arrangement for symmetrically narrowing down the slit, and had an aperture of 65 millim. and a focal length of 1 metre; the observing-telescope an aperture of 75 millim. and a focal length of 1 metre. The grating had a divided surface of 46×36 millim. and possessed very good definition. The slit was made very narrow. For the determination of the maximum of brightness a totally-reflecting prism was placed before one half of the slit; the beam of light from an arc lamp fell upon the uncovered half of the slit, after having traversed two adjustable Nicols. The light from the sodium flame or amyl-acetate lamp entered the apparatus through the totally-reflecting prism. First the sodium flame was brought before the prism, the apparatus adjusted for the greatly expanded sodium lines of the fourth spectrum by turning the grating, and the spectrum of the arc lamp weakened by turning one of the Nicols until it appeared of equal brightness with the sodium lines in that part of the spectrum.

Then the sodium flame was replaced by the amyl-acetate lamp, and adjusted for the yellow of the first diffractive spectrum. Since the light of the electric lamp was much brighter than that of the amyl-acetate lamp it was weakened by clouded glasses of known strength till the two spectra were of equal brightness. A direct comparison of brightness between the amyl-acetate lamp and the sodium flame in the fourth spectrum was not possible, because in the fourth spectrum the first was hardly visible. But since for all sources of white light the degree of weakening in passing from one spectrum to another is the same, and all continuous spectra are dispersed in the same degree, the ratio of brightness of amyl-acetate to arc-light must be the same in the

spectra of different orders. This is also confirmed by a simple experiment. If an equality of brightness between the arc-light and the amyl-acetate light had been established in the first spectrum by the use of clouded glasses, this equality was also observed in the second and third spectra.

The comparison in brightness between the arc lamp and amyl-acetate lamp was made at a point a little distance from the yellow, on the side of the green, so that it might not be disturbed by sodium contained in the arc. The results of experiment are as follows:—The breadth of a sodium line is $\frac{1}{4}$ of the distance between the two lines. The brightness diminishes from the centre rapidly, then more slowly, and at the edges more rapidly again. The measurements were made at about $\frac{1}{4}$ of the breadth from the edge after the brightness of the arc-light had been made equal to that of the sodium flame by rotation of the Nicol, it had then to be reduced to $\frac{1}{3\frac{1}{4}}$ to be of the same brightness as the amyl-acetate lamp.

The brightness of our sodium lamp therefore for rays situated about $\frac{1}{4}$ of the breadth of a sodium line from its edge is 34 times greater than that of the amyl-acetate lamp at the corresponding point.

We may now apply the above results to the experiments on p. 261, where the brightness of the amyl-acetate lamp was compared with that of the sodium flame.

We will take i' to refer to the sodium flame, and i'' to the amyl-acetate lamp. In the sodium double lines the breadth of a single line is $\frac{1}{4}$ of the distance between the centres of the lines, so that the total breadth of the two together is $\frac{1}{2}$ this distance; the distance of the centres is known to be 0.0006μ , consequently the quantity $\lambda_2 - \lambda_1 = 0.0003\mu$; further V was found to be 2.7, since $\Delta = 0.0176\mu$ (p. 259) therefore

$$\frac{i'}{i''} = 2.77 \times \frac{0.0176}{0.0003} = 162.$$

The direct determination for a point about $\frac{1}{4}$ from the edge of the weaker line gave $\frac{i'}{i''} = 34$. It follows from the difference of

these numbers, which agree as to order of magnitude, that the above assumption as to the distribution of brightness is not strictly correct, but that the brightness of a sodium line must increase rapidly in the centre, which is also confirmed by the appearance to the eye. This further indicates the possibility that in spite of the breadth of the spectral lines, interferences may occur with differences of path up to more than 100,000 wave-lengths, such as have been observed by several experimenters.

[To be continued.]

XXXV. *On a Relation existing between the Density and Refraction of Gaseous Elements and also of some of their Compounds.* By Rev. T. PELHAM DALE, M.A.*

IN my former paper which I presented to the Society† I touched upon the empirical relations discoverable between the specific refractive energies of selenium and sulphur. The present state of the data, however, relating to these substances is so far unsatisfactory that we are not furnished with refractive indices and density taken from the same specimen at the same temperature. Thus there is a doubt as to the density to be chosen as normal, in consequence of the difference due to the allotropic conditions in which these substances are found. This difficulty may be to a certain extent eluded by taking both densities and calculating the specific refractive energy for both. The result is, in the case of selenium and sulphur, that in both substances the mean values are not very far apart.

The question, however, of the relation between the refractive indices of the elements is of so great interest, that it appeared advisable to calculate the value of $\frac{\mu-1}{d}$ of all chemical elements for which data existed, giving both refractive index and density in the state of gas or vapour. This would include the refractive indices of hydrogen, oxygen, and nitrogen, and the importance of these is obvious. Then, again, the calculated densities derived from atomic weights might be used in cases where an observation was wanting, and as a check where these existed. A very few trials revealed relations which it was impossible to overlook.

These relations among the numbers found are set forth in the accompanying Tables.

These tables are arranged in columns. The first, column I., contains the name of the substance. Column II. its index of refraction less unity, or $\mu-1$. Column III. its density.

Column IV. its specific refractive energy, or $\frac{\mu-1}{d}$. Column

V. the ratio of $\mu-1$ in the substance to the similar number for hydrogen. The upper row of numbers in each line are natural numbers, and those immediately under are the mantissæ of their common logarithms. Examining this table in detail, it is seen at once that the logarithms of the specific

* Communicated by the Physical Society: read May 25, 1889.

† Phil. Mag. Jan. 1889, p. 50.

refractive energies of nitrogen, chlorine, and phosphorus are nearly identical, as also of sulphur and oxygen. It will also be observed that in N, Cl, P this log. is double of that of H, and S and O three times that of half the log. specific refractive energy of hydrogen. In the same way, the log. of the specific refractive energy of mercury is 8 times, and arsenic 9 times this quantity.

If we turn to column V. we find that oxygen has nearly double the refractive energy of hydrogen, mercury 4, arsenic 8, sulphur, but not so closely, 12 times.

Now all these coincidences arise from observation only, and are independent of theory altogether. The probability that they should be fortuitous is very small in so large a number of instances.

When we turn to the compounds, we again see indications of the same law. Thus N_2O has log. specific refractive energy half of similar log. of CO ; and the refractive energy of Cy is very nearly 6 times that of hydrogen. All these numbers may, roughly it is true, be united under a single law: thus, $\frac{\mu-1}{d}$ for hydrogen = 15696, its logarithm is 19578,

and half this is .09789; this is according to Prof. Everett's data. Mr. Lupton's give .09374. If we multiply these successively by 1, 2, . . . and 10, we shall find that almost all the logarithms range between these two products, being less than the greater and greater than the smaller. It is worth noticing

that $\frac{\pi}{2} = 1.57079$, $\log \frac{\pi}{2} = 19612$ —a curious coincidence which, if quite fortuitous, will nevertheless prove a help to the calculator.

I have not as yet attempted to express these relations under an algebraic formula, although it is obvious that it might be very readily done. I prefer to call attention to the existence of these empirical relations, which hitherto seem to have escaped notice.

In some of the instances set forth in the Tables the density was checked by calculation from the equivalents. This also, it appears, opens another field of great interest. The well-known relations between molecular weight and density lead us to expect a relation between molecular weight and refraction, and this relation has been abundantly worked out in the case of liquids by my friend Dr. Gladstone. I have not as yet had time to work out this part of the subject completely, as though the calculations necessary present no

difficulty they require a numerous and bulky array of figures which must be carefully verified and checked, and also because more data are needed. It is also unfortunate that no data exist, which are accessible to me, of the absorption-spectra of chlorine, bromine, or iodine, or of sulphur and selenium, comparable with the observations here used as the basis of the calculations in the Table. It is in the hope that some of these data may be supplied that I venture to put this paper before the Society.

Some interesting relations are observable between the equivalents and refractions* of gaseous elements and compounds, which I hope to present in a future paper.

Note.—The data are taken from Prof. Everett's 'Units and Physical Constants,' 2nd ed. 1886 (marked E); and from Mr. Lupton's Numerical Tables, 1884 (marked L).

I. Sub- stance.	II. Refraction, $\mu - 1$.	III. Density. d .	IV. Sp. Ref. $\frac{\mu - 1}{d}$.	V. Ratio of Refraction to that of Hydrogen.	Remarks.
H.	0001387 14208	00008837 94630	15696 E 19578	1.00	L 18749 Another value of log. $\frac{\mu - 1}{d}$
O.	2706 43233	14107 14943	19182 E 28290	1.95 29025	L 27923 Lupton's data of log. do.
N.	2977 47378	12303 09318	24241 E 38060	2.14 33170	$\frac{\mu - 1}{d}$ 378134 do.
Cl.	L 7720 88762	E 30909 49009	24986 39753	4.27 62992	} Density given by Everett.
Cl.	L 7720	L 3.18 50243	24274 38159		} Density and Index according to Lupton.
S.	L 16290 1.21192	96 × H 98227	19202 28335	11.7 06984	} Density according to Gmelin.
P.	L 13640 1.13481	62 × H 73869	24895 39612	9.84 99273	} Density calculated from equi- valent.
Hg.	L 5560 74507	100 × H 94630	62918 79877	4.00 60299	} Calculated from hydrogen.
As.	L 11140 1.04689	150 × H 17609	74268 87080	8.03 90479	} Ditto.

* I propose to call the quantity $\mu - 1$ the refraction, μ the index.

Compounds.

I. Sub- stance.	II. Refraction. $\mu-1$.	III. Density. d .	IV. Sp. Ref. $\frac{\mu-1}{d}$.	V. Ratio of Refraction to that of Hydrogen.	Remarks.
N ₂ O.	E 2975 1.47349	E 19433 28554	15309 18495	2.14 33141	All these are calculated from experimental data of both index and density from Prof. Everett.
NO.	E 5159 71257	E 13254 12235	38924 59022	3.72 33141	
CO.	E 3350 52504	E 12179 29024	22947 36720	3.27 57049	
SO ₂	E 7036 84733	E 26990 43120	26070 41613	5.07 70525	
Cy.	E 8216 91466	E 22990 36154	35737 55312	5.92 77258	
NH ₃ .	385 5855	0.761 8814	7041	2.78 4434	6505 Calculated from Lup- ton's numbers.
HCl.	449 6522	1.64 2148	4484	3.24 5107	
H ₂ S.	665 8228	1.52 1818	6410	4.79 6807	
CH ₄ .	443 6464	Calc. 8554	7916	2.02 3040	Density calculated from hy- drogen and equivalent.
C ₂ H ₄ .	678 8312	Calc. 8554	7328	4.9 6900	

These to four places are evidently of less value than those above.

Note by Prof. A. W. RÜCKER, F.R.S., on Mr. Dale's Paper.

It has been shown that the volume of the molecules in unit volume of the substance which they form is $(\mu^2 - 1)/(\mu^2 + 2)$, where μ is the refractive index.

If μ is nearly equal to unity (as in the case of the gases) this expression reduces to $\frac{2}{3}(\mu - 1)$. Hence if δ is the density of the body, v and m the volume and mass of a molecule and n the number of molecules in unit volume,

$$\frac{\mu - 1}{\delta} = \frac{3}{2} \frac{nv}{nm} = \frac{3}{2} \frac{n}{m}.$$

For the same substance the right-hand side should be a constant, and it has been shown that it nearly fulfils this condition.

Mr. Dale now states that for different substances

$$\log \frac{\mu-1}{\delta} = ac,$$

where c is a constant independent of the nature of the substance, and a is an integer. This at once leads to the relation $v/m = \frac{2}{3}e_{ac}$, which would indicate that the ratios of the volumes to the masses of the molecules are in geometrical progression, or, more shortly, the densities of the molecules are in geometrical progression.

If, then, Mr. Dale's conclusions are correct this would be the theoretical inference to be drawn from them.

XXXVI. *On Diffraction-Colours, with special reference to Coronæ and Iridescent Clouds.* By JAMES C. McCONNEL, M.A., Fellow of Clare College, Cambridge*.

[Plate X.]

IN a previous paper† I have explained the occurrence of bright colours in certain clouds near the sun on the hypothesis that the light is diffracted by thin needles of ice or by fine drops of water. In the present paper I give a more complete determination of the actual colours produced, based on Maxwell's observations of the colour-relations of the solar spectrum. The first section contains a mathematical investigation of the light diffracted by clouds of filaments and of spherules respectively. The second is devoted to calculating the colours and setting them out on Maxwell's diagram. And I have been tempted to mark also on the diagram the colours of the sky and sun. In a future number of this Magazine I hope to publish some additional remarks on iridescent clouds and allied phenomena, including Bishop's ring.

1. MATHEMATICAL EXPRESSIONS.

Rectangular Aperture. Point Source.

We will first take the case of a rectangular aperture (sides a b) in an opaque diaphragm inclined to the incident light at an angle γ , and discuss the illumination on a spherical screen of very large radius f , whose centre O coin-

* Communicated by the Author.

† Phil. Mag. November 1887.

cides with that of the aperture. The side a is at right angles to the incident light. In fig. 1 only a quarter of the aperture is represented.

O is the origin of coordinates; the axis of z is drawn towards the source of light, while the axis of x is parallel to the side a of the aperture.

$P(xyz)$ is a point in the aperture.

$M(\xi\eta\zeta)$ a point on the spherical screen.

The source of light is small and very distant.

Let the vibration at O be represented by $\cos \kappa vt$, where t is the time, v the velocity, and $\kappa = 2\pi/\lambda$. The intensity is then unity. The vibration at P is $\cos \kappa(vt + z)$.

We now break up the primary wave into its secondary components over the plane of the aperture, which is not a wave-front. The disturbance at M due to the element $dx dy$ at P is

$$-\frac{dx dy}{\lambda \rho} \sin \kappa(vt + z - \rho)^*, \quad \dots \quad (1)$$

where $\rho = MP$.

Now

$$\rho^2 = (x - \xi)^2 + (y - \eta)^2 + (z - \zeta)^2, \text{ and } \xi^2 + \eta^2 + \zeta^2 = f^2;$$

xyz are small compared with $\xi\eta\zeta$; so, neglecting their squares, we have

$$\rho^2 = f^2 - 2x\xi - 2y\eta - 2z\zeta.$$

In the last term we can put $\zeta = -f$, and obtain

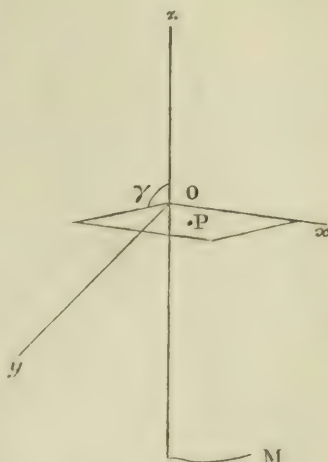
$$\rho = z + f \left(1 - \frac{x\xi + y\eta}{f^2} \right).$$

In the denominator of (1) we may write $\rho = f$. So the vibration at M is

$$-\frac{1}{\lambda f} \iint \sin \kappa \left(vt - f + \frac{x\xi + y\eta}{f} \right) dx dy,$$

the limits of x and y being $\pm \frac{a}{2}$ and $\pm \frac{b \sin \gamma}{2}$ respectively.

Fig. 1.



* *Encyc. Brit.*, art. "Wave Theory of Light," p. 429.

Hence, as usual, the illumination on the screen is given by

$$I = \frac{a^2 b^2 \sin^2 \gamma}{f^2 \lambda^2} \frac{\sin^2 \frac{\pi a \xi}{f \lambda}}{\frac{\pi^2 a^2 \xi^2}{f^2 \lambda^2}} \frac{\sin^2 \frac{\pi b \sin \gamma \eta}{f \lambda}}{\frac{\pi^2 b^2 \sin^2 \gamma \eta^2}{f^2 \lambda^2}} \dots \quad (2)$$

The field is crossed by two sets of parallel dark bands, given by $a\xi/f\lambda$ and $b \sin \gamma \eta/f\lambda$ being positive or negative integers other than zero. There is a large central rectangle of dimensions $2f\lambda/a$ by $2f\lambda/b \sin \gamma$, surrounded by others of similar shape but only a quarter the size. The brightest point is the centre, and along the two principal directions the successive maxima have approximate values 0.046, 0.017, 0.0085, 0.0050..., that at the centre being unity, and are found at distances given by

$$a\xi/f\lambda = 1.43, \quad 2.46, \quad 3.47, \quad 4.47.$$

Along the diagonals the numbers for the successive maxima are the squares of the above, viz. 0.0021, 0.00029, 0.00007..., so the diminution is much more rapid.

It is obvious from (2) that the linear dimensions of the diffraction-pattern are proportional to λ , and its brightness proportional to λ^{-2} . Thus, if the source send out white light, the central patch will be bluish in the middle and tinged with yellow and red at its edges. Along one of the principal directions, *e. g.* $\eta=0$, we have, governing the colour of the light, the factor

$$\sin^2 \frac{\pi a \xi}{f \lambda}. \quad \dots \quad (3)$$

As will be explained in the next section, the colours are those of Newton's rings, though their relative brightness is very different. Along the diagonals for which $a\xi = b \sin \gamma \eta$, the colour-factor is

$$\lambda^2 \sin^4 \frac{\pi a \xi}{f \lambda}. \quad \dots \quad (4)$$

So that the tints are much richer, though the intensity is very feeble.

Babinet's Principle.

Suppose now that $b \sin \gamma$ is much greater than a , so that our aperture becomes a narrow slit. We are intending to deal, not with slits in an opaque screen, but with filaments in the open sky. This case may be immediately derived from the other with the aid of Babinet's principle. But it is desirable, I think, to examine the application of the principle in some detail. Replace the slit by a very much larger aperture, say 500 times as long and 10,000 times as broad. The

screen is so far away that our expression (2) still applies ; in other words, the diffraction-pattern is not supplanted by a geometrical image of the aperture. But the diffraction-pattern is enormously reduced in size, and outside it there is no light. In this outer region the illumination will be the same whether we block up all the aperture except the original slit, or block up the slit by an opaque filament leaving the rest of the aperture open. For the two portions of light must be able to neutralize each other. So we may replace the slit by a filament of thickness a and length b , inclined to the incident light at the angle γ , lying within our large aperture, and (2) will still hold good except within a negligibly small area. If then, according to (3), at any point in the screen the light is, say, green, to an eye placed at that point the aperture will appear a green speck. As the green light is not in any way altered by increasing the size of the aperture, it is clear that it must come from the region immediately surrounding the filament, and that the filament will look green even when the diaphragm is entirely removed.

Cloud of Filaments.

The effect then is the same, whether it be produced by slits in an opaque diaphragm or by filaments in an open space. The calculations are simpler in the case of slits, but practically we have to deal with filaments. So in future we shall speak of filaments only, and in treating of the illumination of the screen we shall refer only to the diffracted light and ignore that which comes direct.

As a is made small compared with $b \sin \gamma$, the diffraction-pattern is stretched out into a long strip, very narrow in the y direction. If there be a large number n of filaments equally inclined to the axis of z and evenly distributed round it, the illumination is found by summing the illuminations due to the individuals. Practically we have to distribute the light we find, according to (2), on any circle round the axis of z evenly over the whole circle and then multiply it by n . Owing to the narrowness of the strip we can treat ξ as constant for points on the circle where the illumination is sensible. So of the three factors on the right-hand side of (2), it is only the last that varies. This third factor may be written $p = \sin^2 u / u^2$, where $u = \pi b \sin \gamma \cdot \eta / f \lambda$. If ξ and $-\xi$ be the points where the circle cuts the plane $y=0$, the average value of p over the whole circle is, remembering that the strip is cut twice,

$$\int p d\eta / \pi \xi.$$

The integration extends over the region for which p is sensible, and we are of course at liberty to extend the limits to $\pm \infty$.

Now $\int_{-\infty}^{+\infty} \frac{\sin^2 u}{u^2} du = \pi$ is a known result, and

$$\pi b \sin \gamma d\eta / f\lambda = du.$$

So

$$\int p d\eta / \pi \xi = f\lambda / \pi b \sin \gamma \xi.$$

Hence the illumination on the screen at points in the plane $y=0$, due to a large number n of equal regularly distributed filaments making an angle γ with the axis of z , is by (2),

$$\frac{na^2b \sin \gamma}{\pi f^2 \lambda} \frac{f \sin^2 \frac{\pi a \xi}{f \lambda}}{\xi \frac{\pi^2 a^2 \xi^2}{f^2 \lambda^2}} \dots \dots \dots (5)$$

To extend this to the case when the filaments are uniformly distributed in all directions we must replace $nb \sin \gamma$ by $\Sigma b \sin \gamma$. If they occupy the fraction α of the field of view looking from the screen, and the summation be extended over an angular area ω equal to that of the sun, we have

$$\alpha \omega f^2 = \Sigma ab \sin \gamma = \alpha \Sigma b \sin \gamma.$$

Now the direct illumination of the sun at the cloud is, by hypothesis, unity, and it has the same value where the observer stands, *i. e.* at the imaginary screen. And it is obvious that the apparent brightness of the sun and cloud are in the same ratio as the illumination due to equal angular areas of each. So, finally, the brightness of the cloud of filaments is in terms of that of the sun,

$$\alpha \omega \frac{af}{\pi \xi \lambda} \frac{\sin^2 \frac{\pi a \xi}{f \lambda}}{\frac{\pi^2 a^2 \xi^2}{f^2 \lambda^2}} \dots \dots \dots (6)$$

On p. 431 of my former article are given expressions for the brightness of the first, second, and fourth bright rings in a cloud of filaments, obtained in a different manner. It will be found on examination that these expressions agree with (6).

In a cloud of filaments, of diameter a , the first four maxima, according to (6), are proportional to 1, 0.215, 0.076, 0.035; the ninth being 0.0036, and the central maximum being infinite. This last result is not surprising, for we have supposed, throughout the greater part of the argument, the source of light to be indefinitely small.

In sunlight the colour is defined by the factor

$$\lambda \sin^2 \frac{\pi a \xi}{f \lambda} \dots \dots \dots (7)$$

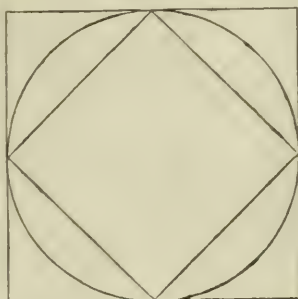
These are the colours produced when the source is a luminous

line and the diffracting aperture a parallel slit, for the blurring is of the same nature as that involved in the transition from (2) to (5).

Diagonals of Square Aperture.

Before giving the accurate expression for the illumination when the diffracting aperture is circular, it will be instructive to examine a case which presents the same peculiarities in an exaggerated form. The main difference between a circle and a square, as regards diffraction in directions parallel to the sides of the square, is that in the former the outlying portions, where the phase-difference is greatest, are relatively small. This feature is still more marked in diffraction by a square parallel to its diagonals (see fig. 2), and this is a case we have incidentally solved.

Fig. 2.



Let c be the diagonal of the square, and ξ the distance of the point on the screen from the centre of the figure in a direction parallel to the diagonal. Then, putting $\gamma = \pi/2$ in (2), we obtain

$$I = \frac{c^4}{4f^2\lambda^2} \frac{\sin^4 \frac{\pi c \xi}{2f\lambda}}{\frac{\pi^4 c^4 \xi^4}{16f^4\lambda^4}} \dots \dots \dots (8)$$

The dark points are given by $c\xi = 2mf\lambda$, where m is any integer other than zero; and in general corresponding points are twice as far out as in directions parallel to the sides of a square of side c . As we have already seen, the diminution of brightness is much more rapid, and the colours, when sunlight is used, are purer.

Circular Aperture.

The expression corresponding to (2) for a circular aperture is

$$I = \frac{\pi^2 R^2}{\lambda^2 f^2} \frac{4J_1^2(z)^*}{z^2}; \dots \dots \dots (9)$$

where

$$z = 2\pi Rr/f\lambda;$$

R is the radius of the aperture, and r the distance on the screen from the centre of the diffraction-figure. The dark

* 'Wave Theory,' p. 432.

rings are given by

$$z/\pi = 1.22, \quad 2.23, \quad 3.24, \quad 4.24, \dots$$

and the maxima of the bright rings are given by

$$z/\pi = 1.63, \quad 2.68, \quad 3.71, \quad 4.72, \dots$$

and have the values

$$0.0175, \quad 0.00416, \quad 0.00160, \quad 0.00078,$$

that at the centre being unity. Thus corresponding parts are rather further out than in the principal directions for a square of side $2R$, and the brightness falls off much more rapidly. It seems legitimate to assume that the colours also, when sunlight is used, are slightly purer.

Cloud of Water-drops.

To pass from the illumination on a screen, due to a single circular aperture, to the brightness of a water-cloud, we follow the lines of the previous argument, with, however, considerable simplification, owing to the orientation of a sphere being a matter of indifference. We have to multiply (9) by the number n of drops within an angular area equal to that of the sun, and this number is given by

$$n\pi R^2 = \alpha f^2 \omega.$$

So the brightness of the cloud, in terms of that of the sun, is

$$\alpha \omega \frac{\pi R^2}{\lambda^2} \frac{4J_1^2(z)}{z^2}. \quad \dots \quad (10)$$

The remarks we have made on (9) apply equally well to (10). The colour-factor in both cases is $J_1^2(z)$. For the two kinds of clouds, compare the values of the maxima given under (6) and under (9). We are enabled to make a fairly complete comparison by the following result. When z is great,

$$J_1^2(z) = \frac{2}{\pi z} \sin^2\left(z - \frac{\pi}{4}\right) \text{ nearly*},$$

giving a colour-factor

$$\lambda \sin^2\left(z - \frac{\pi}{4}\right).$$

Even at the first bright ring the approximation is fair, for it gives the first maximum at $z = 1.716\pi$ with the value 0.0162 ; and it rapidly improves as z increases, though always better at the maxima than at intermediate points. The expression (6) may be written in the form

$$\alpha \omega \frac{a^2}{\lambda^2 z_1} \frac{\sin^2 z_1}{z_1^2},$$

* 'Wave Theory,' p. 432.

where

$$z_1 = \pi a \xi / f \lambda.$$

To secure corresponding points, let

$$z_1 = z - \frac{\pi}{4},$$

and let us choose such values of R and a that the angular distance from the sun is the same ($=\theta$) in both cases. For this,

$$a\theta = 2R\theta - \lambda/4;$$

and, since by hypothesis z is great, $a\theta$ amounts to several wave-lengths; and this equation is satisfied by nearly the same values of a and $2R$ throughout the visible spectrum. The ratio of the brightness of a water-cloud to that of an ice-cloud is then $8R^2 z_1^3 / a^2 z^3$, or $2z_1/z$. Thus the outlying spectra from water-drops are about twice as bright as those from ice-filaments, when the drops and the filaments occupy the same fraction of the field of view, and corresponding spectra are at the same distance from the sun.

Influence of Transparency.

We have incidentally assumed that drops of water and needles of ice can be treated as opaque objects. Now if a single hexagonal filament be placed with one of its faces normal to the sun's rays, it is clear that the light, transmitted through the part that behaves like a parallel plate, must interfere with the light that passes on either side of the filament, and should be taken into account. But with the varying orientation of the filaments, the quantity and relative retardation of the transmitted light would alter to such an extent that the practical result in the case of a cloud would merely be the addition of so much white light.

The case of a spherical drop of water does not admit of the same variety. And it would seem that, when the size is uniform, the transmitted light should be taken into account. The investigation would be complex, even if it be possible with our present knowledge; but we see at once that a large part of the light must be retarded relatively to the uninterrupted light by about a third of the diameter of the drop. Thus the character of the effect would change completely with small changes in the size, and in ordinary clouds we shall not be far wrong in treating the drops as opaque. It is probable that the comparative poorness of water iridescences is partly due to this cause.

The legitimacy of adding the illuminations due to the different drops, without reference to phase, has been shown by

Lord Rayleigh to depend on "the light being heterogeneous, the source of finite area, and the obstacles in motion."

2. CURVES ON THE COLOUR DIAGRAM.

In a very interesting paper (Trans. Roy. Soc. Edinb. July 1886) Lord Rayleigh has set out a curve representing the series of colours of thin plates on Maxwell's form of Newton's diagram. Before such a calculation had been made, it would have been impossible to predict from theory, except in the very roughest manner, the nature of these colours, though the exact composition of the light in terms of wave-lengths were thoroughly known. The reading of this paper made me anxious to obtain a more complete theory of the splendid colours of iridescent clouds, and I have incidentally determined some of the colours of various diffraction-patterns. This led to the discovery of a serious blunder which I made in my former paper on iridescent clouds, in supposing that the central band in the diffraction-pattern of a slit was colourless. I was following high authority, for Verdet says (*Leçons d'Optique Physique*, § 70), "on aperçoit au centre du phénomène une bande blanche et brillante, qui est située sur la direction normale à la fente diffringente." As soon as attention is called to the matter, it is obvious that the edge must be reddish, since the breadth of the band in homogeneous light is proportional to the wave-length; and, as a matter of fact, this red fringe is the finest red of the whole series. The centre of the band is a pale though bright blue. But this depends on the introduction of the factor λ^{-1} in the expression for the secondary vibration, the necessity for which was not recognized in Verdet's time. I do not remember seeing the coloration of the central band distinctly pointed out, though it is implicitly contained in a statement of Verdet in the very section I have quoted. He says that, when white light is used, the red bands correspond to the absence of the brightest part (*i. e.* the yellow) of the spectrum. This is not quite correct, for my results show that they correspond to the absence of the blue-green.

As the three corners of his diagram, Maxwell ("Theory of Compound Colours," Phil. Trans. March 1860) selected equal widths on his prismatic spectrum, near the points marked on his scale by 24, 44, and 68. Between any colour whatever and these three a match can be made by altering the proportions; either a combination of three matching the remaining one, or a combination of two matching a combination of the other two. Thus, for example, unit width at any point of his prismatic spectrum could be expressed as the sum of multiples

of the three units, using negative signs when required. Looking at the table below, we see

$$(36) = 0.48(24) + 1.25(44) - 0.02(68);$$

which means that the mixture of 0.02 of (68) with unit width of (36) is indistinguishable in hue, depth, or brightness from a mixture of 0.48 of (24) with 1.25 of (44). The position of (36) on the diagram is the centre of gravity of weights proportional to 0.48, 1.25, and -0.02 , placed at the three corners (24), (44), and (68). The brightness of a colour is not indicated by the diagram.

The most important property of the diagram is the following. Let us define the brightness of any colour to be the algebraic sum of the corresponding multiples of the three corner units. Then if any colour C be composed of the colour A of brightness α , and of the colour B of brightness β , its position on the diagram is the centre of gravity of weights α at A, and β at B, and its brightness is $\alpha + \beta$. Hence all the colours on any straight line are mixtures of the colours at the two ends of the line, and, in particular, all the points on a straight line drawn from the point white are of the same hue; the depth or purity increasing as we near the spectrum-colours on the borders of the diagram.

Now Maxwell has determined the multiples necessary to express unit width at any point of the spectrum in terms of the three corner units. The sum for the whole spectrum must represent white. And, if the relative brightness of different parts of the spectrum be altered in a known manner, we can, by introducing the proper factors before summation, find the resultant colour in terms of the three units. Lord Rayleigh used a table, containing twenty-two equidistant points of the spectrum, based on Mrs. Maxwell's observations. From this I have deduced the following abridged table, which is sufficiently accurate for my purpose:—

Scale-number.	Wave-length.	(24)	(44)	(68)
20	663	+0.44	+0.04
28	608	+1.17	+0.32	
36	563	+0.48	+1.25	-0.02
44	529	+1.00	
52	500	-0.06	+0.51	+0.28
60	475	-0.05	+0.19	+0.75
68	457	+1.00
76	441	+0.03	+0.69
84	428	+0.33
92	417	+0.15
		+2.01	+3.27	+3.22

The white obtained by superimposing unit widths at the ten points is given by

$$W = 2.01(24) + 3.27(44) + 3.22(68).$$

The chief defect of this table is the omission of the red corner (24). This has been in great measure allowed for by modifying the coefficients for (20) and (28). At the same time the white was brought to practical coincidence with the white of Lord Rayleigh's table.

On the diagram (Pl. X.) are marked the positions of sixteen points equidistant in the prismatic spectrum, from 20 to 80 on Maxwell's scale, with the corresponding wave-lengths. These lie, for the most part, outside the triangle. Rood has determined the places in the spectrum which, when diluted with a suitable amount of white, match the colours of certain pigments ('Modern Chromatics,' p. 38). I had no data for marking the true position of the pigments on the diagram, but their hues (*i. e.* the radii from white on which they lie) are indicated. I have also divided the diagram into five parts,—blue, green, yellow, red, and purple, chiefly in order to name the hues in the "brilliancy" curves described below. In this I have been mainly guided by Rood's 'Modern Chromatics.' On the spectral colours his statements are definite. But the limits of purple, founded on considerations of complimentary colours, are more doubtful. The estimation of hue depends greatly on the brightness of the light and the purity of the colour; and of course, at the best, the lines of division must be rather indefinite. The pure yellow in the spectrum is a very narrow band; so my yellow division consists mainly of orange-yellow and greenish yellow.

In the previous section I have shown that to find the colours in the principal directions of the diffraction-pattern of a rectangular aperture, the proper factor to multiply each of the constituents of sunlight before compounding them is $\sin^2 \frac{\pi a \xi}{f \lambda}$. For the colours of thin plates the appropriate factor, "strictly applicable only to a plate of air bounded by media of small refrangibility," but practically sufficient for all ordinary cases, is $\sin^2 \frac{\pi V}{\lambda}$. Thus identical colours are found in the two cases, whenever the "retardation" V for the thin plate is equal to the extreme retardation $a \xi / f$ of light from one edge of the aperture relative to light from the other. The dotted curve (copied from Lord Rayleigh's) represents these colours, and the small figures at the side are values of $a \xi / f$ expressed, like the wave-lengths, in millionths of a millimetre.

At the central point of the diffraction-pattern ($a\xi/f=0$) the brightness is a maximum (instead of being zero as in the case of a thin plate, when $V=0$), and the colour-factor is λ^{-2} . About 250 the curve passes very near white, on the side towards blue-green. The colour then becomes yellowish, gradually improving, till at 450 a very fine orange is attained. And so on through the well-known series. When the retardation is large the curve approaches nearer and nearer to white, and, in the case of diffraction, the brightness diminishes indefinitely.

Along the diagonals of the pattern, where the colours are the purest, the factor is

$$\lambda^2 \sin^4 \frac{\pi a \xi}{f \lambda}.$$

I have determined two points on the curve, D_1 in the first red ($a\xi/f=500$), and D_2 in the second green ($a\xi/f=810$). The curve starts from the point λ^{-2} , and afterwards for several sweeps keeps outside of the thin-plate curve. The first orange-yellow and the second blue of the latter admit of but little improvement. But in the first red, which borders the central spot, the diagonals are far superior to the principal directions.

We now come to the main object of the present paper, the colours of iridescent clouds, formed by needles of ice. The colour-factor is

$$\lambda \sin^2 \frac{\pi a \xi}{f \lambda}.$$

Comparing this with the factor for thin plates we see that the greater wave-lengths have an advantage. So the curve is, on the whole, displaced from the violet and towards the red. This curve is laid down on the diagram with a continuous line. Points actually determined are marked with small crosses, with the values of $a\xi/f$ annexed in bold figures*. The rest of the curve has been drawn by comparison with the thin-plate curve. The curve starts at the point λ^{-1} , so the central blue though bright is very impure. On the whole the colours are superior to those of thin plates. The reds are distinctly better, especially the third red. The second blue is nearly as good, though the third is decidedly inferior. The third and fourth greens are about on a par for purity, but more inclined to yellow, while the first and second yellows

* The calculations were inadvertently made for $\lambda^2 \sin^2 (\pi a \xi / f \lambda)$, and the points given are put halfway between those thus found and the corresponding points on Lord Rayleigh's curve. All the calculations were made with a slide rule reading to $\frac{1}{3} \frac{1}{10}$.

are somewhat purer and more inclined to orange. The curve ultimately circles round closer and closer to the point λ , a very pale orange-yellow.

The custom of speaking of the successive diffraction spectra is apt to lead to the impression that each spectrum is purest in the middle when it does not overlap its neighbours. In the colours of the first two orders the exact contrary is the fact. A better idea of the phenomenon is arrived at by considering the wave-lengths that are absent; in other words, by considering the dark bands in the spectrum into which each colour could be drawn out. The fine yellows of the first two orders are due to the upper part of the spectrum being nearly quenched by broad dark bands, which as they proceed down the spectrum give the blues of the second and third orders. Before we can obtain a good green we must have two bands to blot out both ends of the spectrum. This occurs at 1330 and 1830.

Fraunhofer (Verdet § 70) using white light measured the deviations of the red bands in the diffraction-image of a slit, and, finding they were in the ratio 1:2:3 . . ., thought he had discovered the law for the successive maxima of homogeneous light. The complete explanation of this may be seen in the diagram; for the points 500, 1000, and 1500, corresponding to the absence of wave-lengths in the neighbourhood of 500, lie almost on the line from W to the red corner. The fourth red was probably not measured by Fraunhofer. The real maxima for wave-length 631 are at the points 0, 900, 1550, 2190.

Maxwell's colour diagram gives us complete information as to the hue and depth of each tint, but is silent as to the brightness; and with cloud colours, which are necessarily more or less contaminated with white light, the brightness is of great importance. It is clear, too, that the power of withstanding contamination depends on the depth as much as on the brightness. It occurred to me, therefore, that it would be instructive to draw a curve with retardations as abscissæ, in which the ordinates should depend on both these qualities, and should represent what I will call the brilliancy of the colour. I have used the following principles:—(1) the brilliancy of white light is zero; (2) the brilliancy of standard red light is reckoned equal to that of standard green or violet light, when they are in the proportion in which they occur in white light; (3) the brilliancy of any colour which is composed of two standard colours is equal to the brilliancy of the more brilliant component. The third principle ensures that the brilliancy of complementary colours should be equal. As an

example of the application, suppose that the red, green, and violet components of two colours are $\cdot536, \cdot338, \cdot042$; $\cdot265, \cdot114, \cdot084$ respectively. The components of white are $2\cdot01, 3\cdot27, 3\cdot22$. Reducing the first two triplets in proportion to the components of white, we have $\cdot267, \cdot104, \cdot013$; $\cdot132, \cdot035, \cdot026$. These may be considered as mixtures of two standard colours with some white light. Subtracting the white light, and taking the greater of the remaining components of each, we find the brilliancies are in the ratio $\cdot254 : \cdot106$. Treated in this manner the brilliancies of the ten points in the spectrum I have chosen come out proportional to 20, 47, 39, 30, 18, 26, 31, 19, 10, 5. The maxima fall in the orange and the blue-violet. The intermediate minimum is in the green-blue, a part of the spectrum where the colour is generally considered poor. It seems probable that, when nearly swamped with white light, the colours would assert themselves nearly in proportion to their brilliancy. At the worst the brilliancy curves will be useful for comparing colours of similar hues.

In Plate X. is given a curve representing the brilliancy of iridescent ice-clouds in accordance with the expression (6). Owing to the occurrence of ξ in the denominator the brilliancy is infinite when ξ is zero, and decreases rapidly as ξ increases. The ordinates of the latter portion of the curve are drawn on a scale five times as great as those of the former. The points where the nearest approach is made to the pure colours of the spectrum are marked by the letter p . Under the most favourable circumstances in the clouds, when all the filaments are of the same size, there are two important causes of blurring. The first is the finite diameter of the sun, which, of course, prevents the brilliancy from being infinite. When the first purple is at 5° from the sun—about an average distance—the colours over a range of 40 of retardation will be all mixed together. This effect will be less marked when the particles are finer and the colours further out. Another cause, more serious than the other, especially for large retardations, is the effective diameter of the filaments varying from 1 to $1\cdot155$.

Let us now deduce the successive colours from a study of the two diagrams, assuming the particles are of such a size as to give 100 of retardation to a degree of arc. Up to 1° or $1\frac{1}{2}^\circ$ from the centre of the sun the light is very bright and of a perceptibly bluish hue. If the cloud be dense even the face of the sun will be tinted blue. From $1\frac{1}{2}^\circ$ to $2\frac{1}{2}^\circ$ the light is practically white. Then a yellowish tinge asserts itself, which attains its greatest brilliancy at $3\frac{1}{2}^\circ$ and its greatest purity at $4\frac{1}{2}^\circ$. Between this and $5\frac{1}{4}^\circ$ intervenes a narrow ring of

reddish orange. Then a bluish purple extends as far as $5\frac{3}{4}^\circ$. A broad band of blue reaches to $7\frac{1}{4}^\circ$, a poor green to $8\frac{1}{4}^\circ$, a fine yellow to $9\frac{1}{2}^\circ$, an orange-red to $10\frac{1}{3}^\circ$, a reddish purple to $11\frac{1}{4}^\circ$. The third blue (to $12\frac{1}{2}^\circ$) is greener and decidedly poorer than the second. The third green (to $13\frac{1}{2}^\circ$) is much inclined to yellow. The next noticeable colour is in the pink at $15\frac{1}{2}^\circ$. There is a faint green at $18\frac{1}{2}^\circ$, and a faint pink at $21\frac{1}{2}^\circ$.

On Feb. 25th last I noted down some rather fine colours in ice-clouds, in which the tint seemed to depend mainly on the distance from the sun; in order outwards, yellow, bright red, purple, green, greenish yellow, faint pink. A few minutes later the purple had altered to faint purple, bright blue, and the outer pink was succeeded by purple and green. This is a good illustration of the extent to which the theoretical colours are realized in observation.

Partly for the sake of comparison and partly on account of its intrinsic interest, I give the curve of brilliancy of thin-plate colours, deduced from Lord Rayleigh's figures, with the addition of an ordinate I have calculated in the first blue. The light is supposed to fall at a uniform angle on a film of varying thickness.

When the diffracting particles are spherical the colour-factor is, as we have seen, $J_1^2(z)$. When z is indefinitely small $J_1(z) = z, 2$; so the curve starts from the point λ^{-2} . When z is great

$$J_1^2(z) = 2\pi^{-1} z^{-1} \sin^2 \left(z - \frac{\pi}{4} \right);$$

so the curve starts somewhat outside the filament curve and after a time comes near coincidence with it, finally oscillating about the same point λ . I have calculated the colour* for $2Rr/f = 600$. This is the point marked C_1 on the diagram, which happens to fall exactly on the filament curve. I think we may conclude that from the first red upwards the colours produced by filaments and by drops will be practically identical.

In the previous section I have shown that for a not too small distance θ from the sun and for corresponding colours, *i. e.* when

$$a\theta = 2R\theta - \lambda/4,$$

the brightness of the water-cloud is about twice that of the ice-cloud. Thus we may make a fair approximation to the brilliancy curve of the former beyond the first purple by

* Using the table for $\phi(n) = 2\pi^{-1}J_1(n)$ given by Airy at the end of 'The Undulatory Theory of Optics.'

letting the curve, hues included, stand as it is, and pushing the abscissæ to the left through a distance $\lambda/4$. The quarter wave-length varies from 158 at the red corner to 114 at the violet corner; but it is sufficient to take the mean 136. Even in the first red we find $2R\theta = 600$ corresponding to $a\theta = 485$.

An easy way of seeing these colours to advantage is to lightly sprinkle the object-glasses of a pair of field-glasses with lycopodium seed and direct them to the neighbourhood of the sun. The pooriness of the green of the second order compared with that of the third order is well brought out, also the blueness of the first purple compared with the second. The green of the fourth order is quite distinct, and the corresponding red just visible.

The most notable difference between the colours of ice-filaments and those of water-drops is the superiority of the first blue of the latter both in purity and extent. On the whole this agrees with observation, for the best inner blues that I have seen in water-clouds were superior to the best inner blues in ice-clouds.

Colours of the Sky and Sun.

To lend additional interest to the diagram I have calculated a few points representing these colours. It is now certain that the blue of the sky and the reddish tinge of the setting sun are mainly due to the scattering of light by particles small compared with a wave-length. The theory of this action is due to Lord Rayleigh*. All that we require for our present purpose is the law that the scattered light varies inversely as the fourth power of the wave-length. When the various parts of the spectrum are compounded in this proportion, we obtain the point marked λ^{-4} on the diagram. This is a fair approximation to the blue of the sky near the zenith. Lord Rayleigh's preliminary measures gave the sky a somewhat richer hue.

Since the scattered light varies as λ^{-4} it may be shown that the transmitted light must vary as $e^{-kx\lambda^{-4}}$, where x is the length of path and k is a constant, depending on the size and material of the particles and on their number in a given space. The particles will, on the whole, be more numerous where the air is denser, and it is reasonable to take x proportional to the mass of air traversed. Capt. Abney has found that if x be expressed in atmospheres and λ in thousandths of a millimetre ($\lambda_D = 0.589$), k has the value

* Phil. Mag. Feb., April, June, 1871, Aug. 1881.

0.013 *. An atmosphere is defined to be the mass of air traversed by a line drawn vertically upwards from the level of the sea. The value depends on two series of observations on particularly fine days at South Kensington, when the air-thicknesses were about 1.3 and 3.3 atmospheres. Taking the colour of the sun outside the atmosphere as the point W, the points S_5 , S_{10} , S_{20} , S_{40} on the diagram give the colour of sunlight which has traversed 5, 10, 20, and 40 atmospheres respectively. The first two are yellow inclining to orange, the third a fine orange, and the fourth redder than red lead. To the colour of the fourth, wave-lengths less than 529 contribute nothing appreciable; and even in the third the violet sensation is mainly due to wave-length 663. For an observer at sea-level the first three thicknesses occur when the apparent zenith distance of the sun is $78\frac{1}{2}^\circ$, 85° , and $87\frac{3}{4}^\circ$. For apparent Z.D. 90° the thickness is 35.5 atmospheres †. The additional 4.5 atmospheres can be secured by ascending a height of 330 feet, while from a height of 3000 feet the coloration due to 50 atmospheres can be studied. The same action is exhibited to some extent by clouds near the horizon and by distant snow mountains. For example, the Alps seen from Berne, forty miles away, look yellowish. But here the colour is interfered with by the intervening "blue sky." In other words, the particles, which sift the blue waves out of the light from the snow, send to the observer a by no means negligible quantity of scattered sunlight.

It is only when the colour of the sun is white that the sky is represented by λ^{-4} . If sunlight be represented by S_5 , then skylight will be represented by σ_5 , slightly on the green side of white. The paleness of the sky, when the sun is low, is a familiar phenomenon. Similarly σ_{10} , σ_{20} , correspond to S_{10} , S_{20} . But it is clearly of no consequence whether the shorter wave-lengths are filtered out before or after scattering. So if we could look at the ordinary blue sky through a tube, filled with air, 25 miles long it would appear pale greenish white. In the same way the blue of the sky near the horizon is of poorer quality than near the zenith. When the scattered light either before or after scattering has had to traverse 40 atmospheres, its colour reaches the point σ_{40} on the diagram, i. e. it is really red. This is the red of a sunset sky. It is to be noticed that the form of the curves W, S_5 , S_{10} , S_{20} , S_{40} , and

* Phil. Trans. 1887. His statements left me in some doubt as to the position of the decimal points, but the evidence of his diagram was decisive.

† From Forbes's values, quoted by Abney, which allow for refraction and the curvature of the earth.

λ^{-4} , σ_5 , σ_{10} , σ_{20} , σ_{40} is given by the theory, but to find the position of these points on the curves we require to know the value of k .

Since the triangle in my diagram is equilateral, the colour represented by any point P within the triangle can be experimentally obtained in the following way :—Let a prismatic spectrum fall on a diaphragm with three adjustable slits, whose centres are at wave-lengths 631, 529, and 457. Make the breadths of the slits proportional to the perpendiculars drawn from P to the sides of the triangle. Then the three spectral rays, when compounded by a lens, will produce the colour P. The dispersion of Maxwell's spectrum is defined by the wave-lengths I have given of the sixteen equidistant points in his spectrum. In consequence, however, of individual variations in the colour sensations great accuracy would be thrown away.

Hotel Buol, Davos, July 10th.

XXXVII. *On the Molecular Constitution of Isomeric Solutions &c.* By Dr. G. GORE, F.R.S.*

IN the present research, the "voltaic balance" has been applied to the detection of differences of chemical constitution of a pair of isomeric solutions; and to detect molecular and chemical changes in them, caused by heat, light, lapse of time, order of mixture, degree of dilution of ingredients, &c.

According to the results of J. Thomsen's thermochemical investigations, as described by P. Muir ('Principles of Chemistry,' 1884, pp. 434, 437), "when nitric acid and sodium sulphate react in equivalent quantities in a dilute aqueous solution, heat is absorbed; but when sulphuric acid and sodium nitrate react under similar conditions, heat is evolved. But the final distribution of the base between the two acids will be the same in both cases, and, moreover, this distribution will be the same when equivalent quantities of the two acids (sulphuric and nitric) and the base (soda) mutually react." "When soda, nitric acid, and sulphuric acid mutually react in equivalent quantities in a dilute aqueous solution, two thirds of the soda combines with the nitric acid, and one third with the sulphuric acid." "The final division of the base between the two acids is the same whether the soda were originally present as sulphate or nitrate." (See also 'Theories

* Communicated by the Author.

of Chemistry,' by L. Meyer, translated by Bedson and Williams, 1888, pp. 470, 485.)

Experiment 1.—I have examined this instance by means of the "voltaic-balance" method with zinc and platinum couples (see Roy. Soc. Proc. vol. xlv. pp. 265, 268), and have obtained the following results. Distilled water was used in making all the solutions.

TABLE I.

						Voltaic energy.
"A."	$\text{Na}_2\text{SO}_4 + 2\text{HNO}_3$	gave between	73,313 and 81,579	at 18° C.	Average	77,446
"B."	$2\text{NaNO}_3 + \text{H}_2\text{SO}_4$	"	31,000	"	34,444	32,722

The solutions of each ingredient of these two mixtures were considerably diluted previous to mixing.

The numbers obtained with the mixture "A" are much more variable than those obtained with the one "B," and it will facilitate the clear understanding of the subsequent parts of this research if I here state that the mixture "A" is an unstable one, and liable to change in molecular constitution and amount of energy both during its formation and afterwards.

It is worthy of notice that notwithstanding the average voltaic energy of sulphuric acid in water is about 3·9 millions, and that of nitric acid in water is only about 3·2 millions (see Table II.), the mixture "A" containing the latter acid has about 2·3 times the amount of such energy of the one "B" containing sulphuric acid: this is probably explained by the changes of energy which occur during mixing.

The amounts of energy show that the distribution of acids and base in the two isomeric liquids "A" and "B" were very different. It is evident that if one of the mixtures consists of "two thirds of the soda combined with the nitric acid, and one third with the sulphuric acid," the other liquid must have a very different molecular arrangement; and that "the final division of the base between the two acids" is not always "the same whether the soda were originally present as sulphate or nitrate." The amounts of voltaic energy, however, appear consistent with the statement that "when nitric acid and sodium sulphate react in equivalent quantities in aqueous solution, heat is absorbed; but when sulphuric acid and sodium nitrate react, under similar circumstances, heat is evolved." The mode of preparing each liquid will be described.

In each of these two solutions the following compounds may possibly be present:— $\text{H}_2\text{SO}_4 - \text{HNO}_3 - \text{Na}_2\text{SO}_4 - \text{NaNO}_3 - 2\text{HNO}_3$, $\text{H}_2\text{SO}_4 - \text{Na}_2\text{SO}_4$, $\text{H}_2\text{SO}_4 - 2\text{NaNO}_3$, $\text{H}_2\text{SO}_4 -$

Na_2SO_4 , $2\text{HNO}_3-\text{NaNO}_3$, HNO_3 — and 2NaNO_3 , Na_2SO_4 ; besides the more complex aggregates and the total aggregate formed by the feebleness of the chemical union of these compounds with each other (see Table IX.; also “A Method of Detecting dissolved Chemical Compounds and their Combining Proportions,” Roy. Soc. Proc. vol. xlv. p. 265). The following are the relative amounts of voltaic energy of some of these substances:—

TABLE II.

				Voltaic energy.
H_2SO_4 .	Between 3,690,476 and 4,111,466 at 19° C. Average			3,900,941
HNO_3	3,030,215	3,369,565	..	3,204,295
Na_2SO_4	1,914	2,126	13	2,020
NaNO_3	155	177	12	163
NaHO (80.04+19.96 } per cent. Na_2CO_3) . }	-254,934	-287,037	17	-270,985

Speaking of thermochemical measurement in this case, L. Meyer states “according to J. Thomsen’s experiments, although the action of one acid upon the other, and the action of the salts on each other, do not produce any effect capable of measurement, still each acid produces a greater or less thermic effect with its own salt.” And in the case of decomposition of a salt by an acid, he says “the extent of the decomposition can be determined from the value of the thermic effect.” “But for this determination it is necessary to make an extensive series of experiments, showing the thermochemical effect of each pair of the substances in question. The action of each acid on the base must be separately determined, then the action of each acid on its own salt, and also on the salt of the other acid, and finally the mutual action of the two acids.” “The greatest possible care has to be exercised in each individual determination, in order that the result may be trustworthy; if due caution is not observed utterly false results are easily obtained.” “Secondary thermic results are also produced by the mutual action of the other bodies.” (‘Modern Theories of Chemistry,’ pp. 466–468.) According to J. Thomsen, the value of the thermal change attending the reaction of dilute sulphuric upon dilute nitric acid “is so small that it cannot be accurately determined” (‘Principles of Chemistry,’ p. 435).

If, however, we employ the “voltaic-balance” method instead of the thermochemical one, not only the chemical union of each of the acids with each of the salts, and with one another, and the individual salts with each other, is clearly shown by a depression of energy, but even that of Na_2SO_4 , 2HNO_3 , with 2NaNO_3 , H_2SO_4 , and of still more complex

aggregates with each other are indicated. The numerical results contained in the following Tables support this statement. Each definite compound formed is the one having the smallest amount of voltaic energy, and its formula is indicated by a star (*).

The chemical union indicated by the minimum amount of voltaic energy in Tables III. to IX. is a distinct phenomenon from the chemical and thermal changes which occur during mixing the constituent solutions of each pair of substances.

TABLE III.— $\text{HNO}_3 + \text{H}_2\text{SO}_4$.

							Voltaic energy.
$3\text{HNO}_3 + 2\text{H}_2\text{SO}_4$.	Between	3,604,651	and	3,900,000	at 11°C .	Average	3,752,325
4 " +2 " *	"	3,000,000	"	3,300,000	"	"	3,150,000
5 " +2 "	"	3,555,046	"	3,780,487	12	"	3,204,295

TABLE IV.— $\text{Na}_2\text{SO}_4 + \text{HNO}_3$.

$2\text{Na}_2\text{SO}_4 + 5\text{HNO}_3$.	Between	81,579	and	91,176	at 18°C .	Average	86,377
2 " +4 " *	"	73,313	"	81,579	"	"	77,446
2 " +3 "	"	84,700	"	92,261	"	"	88,480

TABLE V.— $\text{NaNO}_3 + \text{H}_2\text{SO}_4$.

$3\text{NaNO}_3 + 2\text{H}_2\text{SO}_4$.	Between	33,695	and	37,440	at 18°C .	Average	35,564
4 " +2 " *	"	31,000	"	34,444	"	"	32,722
5 " +2 "	"	35,469	"	39,440	"	"	37,454

TABLE VI.— $\text{Na}_2\text{SO}_4 + \text{H}_2\text{SO}_4$.

$4\text{Na}_2\text{SO}_4 + 5\text{H}_2\text{SO}_4$.	Between	31,900	and	35,477	at 12°C .	Average	33,689
4 " +4 " *	"	29,245	"	32,291	"	"	30,768
4 " +3 "	"	32,631	"	36,046	"	"	34,338

TABLE VII.— $\text{NaNO}_3 + \text{HNO}_3$.

$4\text{NaNO}_3 + 5\text{HNO}_3$.	Between	24,603	and	27,200	at 12°C .	Average	25,901
4 " +4 " *	"	22,142	"	24,603	"	"	23,372
4 " +3 "	"	24,603	"	27,200	"	"	25,901

TABLE VIII.— $\text{NaNO}_3 + \text{Na}_2\text{SO}_4$.

$3\text{NaNO}_3 + 2\text{Na}_2\text{SO}_4$.	Between	79	and	88	at 12°C .	Average	83.5
4 " +2 " *	"	70.4	"	79	"	"	74.7
5 " +2 "	"	79	"	88	"	"	83.5

TABLE IX.— $\text{Na}_2\text{SO}_4, 2\text{HNO}_3 + 2\text{NaNO}_3, \text{H}_2\text{SO}_4$.

$3(\text{Na}_2\text{SO}_4, 2\text{HNO}_3) + 4(2\text{NaNO}_3, \text{H}_2\text{SO}_4)$.	Between	2,583	and	2,870	at 13°C .	Average	2,726
4(") + 4(") *	"	2,280	"	2,541	"	"	2,410
5(") + 4(")	"	2,541	"	2,818	"	"	2,679

It has already been shown by the voltaic-balance method, that the action of chemical affinity between substances in aqueous solution is not limited to small groups of a few dissimilar kinds of molecules, such as those represented in Tables III. to VIII., but extends to large aggregates composed of a variety of molecules, the aggregates being apparently without limit of magnitude or variety, but subject to the law of chemical equivalence. The results given in Table IX. further support this statement. The complex structure in such cases is usually built up by making each addition chemically equivalent to the whole of the previously existing compound (see "A Method of Detecting dissolved Chemical Compounds and their Combining Proportions," Roy. Soc. Proc. vol. xlv. p. 265; also "The Loss of Voltaic Energy of Electrolytes during Chemical Union," Proc. Birm. Phil. Soc. vol. vi. part 2).

Loss of Voltaic Energy during Mixing.

In order to arrive at the loss of voltaic energy which took place during the mixing of the two constituents in each of the above cases, the average energy of each constituent was multiplied by its chemical equivalent, and the two amounts added together to obtain the total amount of energy of the constituents. The average energy of the compound was then multiplied by its molecular weight, to arrive at its total voltaic energy, and the product subtracted from the total voltaic energy of its constituents. The following are the results:--

TABLE X.

	Total loss.	Loss Per cent.
$2\text{HNO}_3 + \text{H}_2\text{SO}_4$	80,433,388	= 10.23
$2\text{NaNO}_3 + \text{Na}_2\text{SO}_4$	291,244	= 91.49
$\text{Na}_2\text{SO}_4 + 2\text{HNO}_3$ (Mixture "A") .	383,262,482	= 94.86
$2\text{NaNO}_3, \text{H}_2\text{SO}_4 + \text{Na}_2\text{SO}_4, 2\text{HNO}_3$	28,233,264	= 95.62
$2\text{NaNO}_3 + \text{H}_2\text{SO}_4$ (Mixture "B")...	373,550,432	= 97.70
$\text{Na}_2\text{SO}_4 + \text{H}_2\text{SO}_4$	375,154,338	= 97.81
$\text{NaNO}_2 + \text{HNO}_3$	198,425,384	= 98.49

In each of these instances the loss of voltaic energy appears to be due to chemical union of the two dissolved substances.

Influence of Proportion of Ingredients upon the Amount of Energy.

With the object of obtaining graphic representations of the influence of proportion of ingredients, the following series of measurements were made. With each mixture, 1 part by weight of each ingredient was diluted with not less than 1550 parts of water previous to mixing. The proportions of the substances employed are stated in the form of molecular weights. Only the "average" amounts of voltaic energy are given.

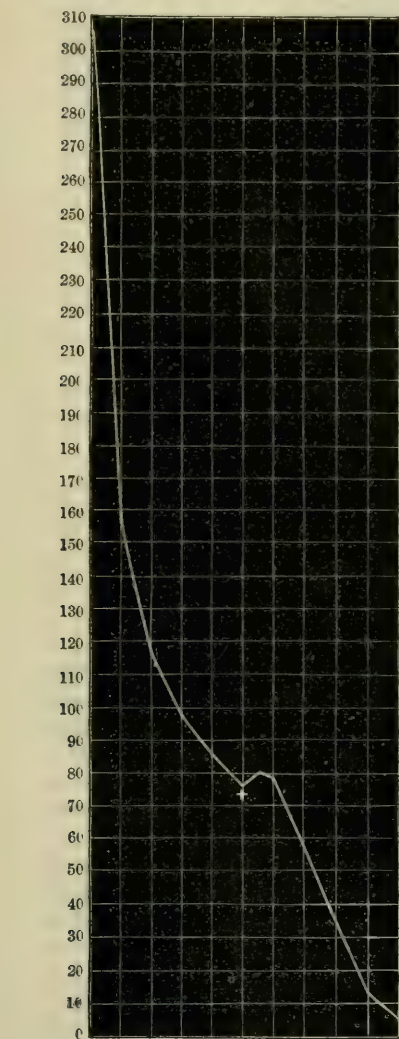
TABLE XI.

<i>Exp. 2.—Mixture "A."</i>				<i>Exp. 3.—Mixture "B."</i>			
HNO_3 at 19°C.				H_2SO_4 at 19°C.			
$1\text{Na}_2\text{SO}_4 + 12$	"	21	3,204,295	$2\text{NaNO}_3 + 6$	"	"	3,900,941
2 "	+12	"	305,045	4 "	+6	"	136,342
3 "	+12	"	157,318	6 "	+6	"	56,724
4 "	+12	"	16,864	8 "	+6	15	43,194
5 "	+12	23	99,031	10 "	+6	21	35,934
6 "	+12*	"	86,377	12 "	+6	"	33,429
6 "	+12*	"	75,880	12 "	+6*	19	30,872
6 "	+11	20	81,805	12 "	+5½	20	35,686
6 "	+10	"	79,106	12 "	+5	"	31,250
6 "	+8	23	56,725	12 "	+4	21	27,268
6 "	+6	21	34,603	12 "	+3	19	20,813
6 "	+4	"	14,874	12 "	+2	21	13,568
6 "	+2	"	9,364	12 "	+1	"	5,453
Na_2SO_4		13	2,020	NaNO_3		12	163

Each of these two series shows the depression of energy at the combining proportion, attending complete chemical union of the two substances. The excess of either ingredient appears to exist largely as uncombined mixture. The two chief causes which appear to determine the magnitudes of voltaic energy are, strength of chemical union of the dissolved substances with zinc, and dilution of the definite compound by the substance in excess; at the combining proportion the latter influence does not exist because there is no substance in excess. In the upper part of each series these two causes cooperate, whilst in the lower part they counteract each other: in the upper part, the stronger substance being in excess, enlarges the magnitudes; in the lower part the weaker one is in excess, and diminishes them. The influence of dilution appears to preponderate over that of chemical union generally in each series. Variation of amount of excess of acid has in each series a much greater effect than variation of excess of salt. The following are the curves representing the above numbers:—

Mixture "A."

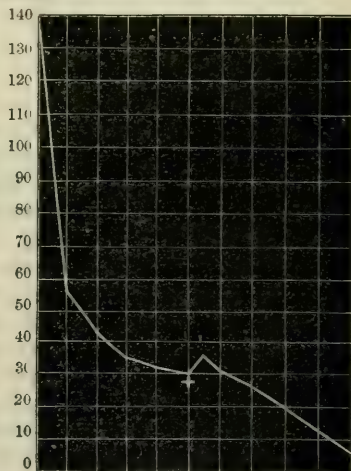
1000nds.



HNO₃. 12 12 12 12 12 12 11 10 8 6 4 2
Na₂SO₄. 1 2 3 4 5 6 6 6 6 6 6 6

Mixture "B."

1000nds.



H₂SO₄. 6 6 6 6 6 6 5½ 5 4 3 2 1
NaNO₃. 2 4 6 8 10 12 12 12 12 12 12 12

Influence of Degree of Aqueous Dilution upon the Molecular Constitution &c.

Mixture "A."

Experiment 4.—Six solutions were made, each containing 1 molecular weight proportion of anhydrous sodium sulphate; and six others, each containing 2 of nitric acid. These were first diluted to different degrees, and then mixed to form six liquids of the following degrees of strength :—

TABLE XII.— $\text{Na}_2\text{SO}_4 + 2\text{HNO}_3$.

No. 1 contained	1 grain of the mixture in	10·34 grains of water.
" 2	1	15·50
" 3	1	155·00
" 4	1	310·00
" 5	1	1,550·00
" 6	1	15,500·00

Each of these solutions had to be further diluted previous to measuring their energy. The amounts of their voltaic energy were as follows :—

TABLE XIII.

No. 1.	Between 30,511 and 33,917 at 12° C.	Average 32,214.
" 2.	"	"
" 3.	32,493 " 35,714 " 12·5	" 34,103.
" 4.	41,005 " 45,600 " 13	" 43,302.
" 5.	70,457 " 77,500 " 12	" 73,977.
" 6.	73,810 " 81,578 " 11	" 77,694.

The mixture which was formed by adding together the weakest solutions gave the greatest energy and the same amount as "A" in Table I.; and the ones formed from the two strongest solutions gave the least energy and the same as "B" in that table. The only way in which I have been able to form the isomeric liquid "A" has been by first mixing very dilute cold solutions in the manner just described.

It is evident from the numerical results, that *the degree of strength of the original solutions* of sulphate of sodium and nitric acid *at the moment of mixing* largely affected the molecular structure, the distribution of acids and base, and the amount of voltaic energy of the resulting mixture; and that these were more or less determined or fixed at that moment, and were not rendered alike in the different cases by the subsequent dilution necessary for the voltaic measurement. Similar results occurred with a mixture of potassic iodide and chlorine (Roy. Soc. Proc. vol. xlv. p. 440). Probably the smaller amounts of voltaic energy of the liquids made from the more concentrated original solutions were due to more

energetic chemical action occurring at the moments of mixing of those solutions than during the mixing of the weaker ones.

Mixture "B."

Experiment 5.—Four solutions, of different degrees of strength, were also made of the isomeric mixture of 2 molecular weight proportions of sodium nitrate and 1 of sulphuric acid, exactly in the same manner as those of "A."

TABLE XIV.— $2\text{NaNO}_3 + \text{H}_2\text{SO}_4$.

No. 1	contained	1 grain	of the mixture in	15.5 grains	of water.
" 2	"	1	" "	155.0	" "
" 3	"	1	" "	1,550.0	" "
" 4	"	1	" "	15,500.0	" "

The amounts of voltaic energy given by these, after suitable dilution, were :—

TABLE XV.

No. 1.	Between	30,511	and	33,917	at 15° C.	Average	32,214.
" 3.	"	31,000	"	34,444	" 18	"	32,722.
" 4.	"	30,511	"	33,917	" 12.5	"	32,214.

That of No. 2 was not measured. The results show that the mixture of sodium nitrate and sulphuric acid was much more stable than that of sodium sulphate and nitric acid; and that variation of degree of dilution did not change the amount of its voltaic energy, and probably also not the distribution of acids and base in it.

According to the statements, that "two thirds of the soda combines with the nitric acid and one third with the sulphuric acid," and that "the final division of the base between the two acids is the same whether the soda were originally present as sulphate or nitrate," considerable chemical change must have occurred during the mixing of the ingredients of "B" (as well as during that of "A"). And as the amount of voltaic energy of completely decomposed "A" is the same as that obtained at the outset with "B" (see Tables I. and XV.), the latter mixture attains completely its final state during the process of mixing. The thermal phenomena also support this conclusion.

In J. Thomsen's experiments with each of the mixtures "A" and "B," "the quantity of water serving as solvent varied but slightly, so that the results of the experiments only hold good for dilute solutions" ('Theories of Chemistry,' p. 467).

Influence of Order of Mixing.

Experiment 6.—In each of the previous experiments a solution of the salt of sodium was first taken, and then one of the

proper acid added to it ; but in the present case the two dilute acids were first mixed and then a dilute solution of caustic soda added to the mixture.

Three solutions were made, of different degrees of dilution, of a mixture of 1 equivalent each of the two acids ; and three others, of similar degrees of dilution, of 1 equivalent of caustic soda ; the solutions of acid and alkali of corresponding degrees of dilution were then mixed together.

TABLE XVI.

No.	1	contained	1	grain of the mixture in	15.5	grains of water.
"	2	"	1	"	1,550.0	" "
"	3	"	1	" "	15,500.0	" "

Their amounts of voltaic energy were then, after suitable dilution, measured.

TABLE XVII.

No.	1	gave between	30,511	and	33,917	at	15° C.	Average	32,214.
"	2	"	"	"	"	"	"	"	"
"	3	"	"	48,407	"	53,819	"	13	51,123.

From the results of exps. 4, 5, and 6, it appears:—(1) that, with *sufficiently dilute* solutions of the acid and alkali, if the sulphuric acid was first added to the soda and the nitric acid then added to the sulphate of sodium, the voltaic energy of the mixture was about 73,977 ; (2) that with either concentrated or dilute solutions, if the nitric acid was first added to the soda and then the sulphuric acid to the nitrate of sodium, the energy was = 32,722 ; and (3) that with all except very weak solutions, if the two diluted acids were first mixed together and then the soda added to the acid mixture, the energy was also 32,722 ; whilst with very weak ones it was = 51,123. The order of addition of the liquids to each other therefore affects the amount of voltaic energy, the molecular constitution of the liquid, and probably also the distribution of acids and base in it.

Influence of Time.

Experiment 7.—A solution of the mixture of sodium sulphate and nitric acid was prepared from diluted ingredients. It contained 1 grain of the mixture in 1550 grains of water, and its average voltaic energy was = 77,694 ; but after standing in a dark place during 72 hours at about 11° C. its energy was

Between 63,786 and 71,101 at 11° C. Average 67,443.

After standing an additional 48 hours its energy was

Between 57,407 and 63,786 at 12° C. Average 60,596.

The mixture "A" therefore slowly altered in chemical constitution at ordinary temperatures towards that of "B." The one "B" did not change under these conditions.

Influence of Heat.

Mixture "A."

Experiment 8.—Dilute solutions of sodium sulphate and nitric acid were mixed, and the mixture, containing 1 grain of substance in 1550 grains of water, and giving, after the necessary further dilution, the usual average amount of voltaic energy, viz. about 77,000, was heated during two minutes to 100° C. in a closely stoppered glass flask, then cooled, agitated, and its amount of energy measured; it was

Between 30,511 and 33,917 at 15° C. Average 32,214.

The mixture was therefore an unstable one, and its voltaic energy was rapidly and largely reduced by rise of temperature.

Experiment 9.—The solution of the same mixture, containing 1 grain of substance in 15.5 grains of water, the energy of which had already been reduced to 32,214 by insufficient dilution during its preparation (Exp. 4), was heated exactly the same as in Exp. 8; it then gave

Between 30,511 and 33,917 at 15° C. Average 32,214.

Its voltaic energy therefore was unaffected.

Experiment 10.—In order to diminish the amount of chemical change which took place during the mixing of the ingredients of "A," very dilute solutions of them were taken and cooled to 3° C. immediately before mixing. The mixture contained 1 grain of substance in 15,500 grains of water. Its amount of voltaic energy was

Between 91,176 and 100,650 at 11° C. Average 95,913.

The lower temperature therefore diminished the amount of chemical change which occurred during the mixing (compare Exp. 4). This shows that under the ordinary conditions of temperature, when making the mixture "A" some decomposition occurred.

Experiment 11.—A precisely similar experiment was made with less diluted ingredients, forming a mixture of 1 grain of substance in 155 grains of water. Its amount of energy was

Between 30,511 and 33,917 at 11° C. Average 32,214.

The influence of the lower temperature therefore in this case was insufficient to neutralize that of stronger solution,

and did not prevent the maximum amount of chemical change taking place.

Experiment 12.—A mixture, each constituent solution of which contained 1 grain of the substance in 1550 grains of water was heated to 50° C. during two minutes, the liquid cooled, and its energy measured; it was

Between 33,917 and 37,804 at 12° C. Average 35,860.

The temperature therefore was hardly sufficiently high to entirely change the mixture into the fixed product during the given period of time.

The circumstance that, by using either stronger solutions, or heated ones, of the constituents of the mixture "A," the latter yields the same amount of voltaic energy as that given by the final product of the mixture "B," agrees with the conclusion arrived at from thermochemical data that "the final division of the base between the two acids is the same whether the soda were originally present as sulphate or nitrate."

Mixture "B."

Experiment 13.—The solutions of sodium nitrate and sulphuric acid, of the degrees of strength of Nos. 1 and 3 (Exp. 5), were mixed, the mixtures heated to about 100° C. during two minutes, cooled, agitated, and their amounts of energy measured. Each gave the same, viz.

Between 30,511 and 33,917 at 14° C. Average 32,214.

The mixture "B" therefore was evidently completely formed and fixed in chemical constitution at the moment of mixing, and whether the solutions of it were more or less dilute, rise of temperature did not alter their amounts of voltaic energy or their molecular constitution (see Table XV.).

Experiment 14.—In this experiment, the two diluted acids were first mixed, and heated to nearly 100° C.; a dilute solution of caustic soda, equivalent in amount to one of the acids, and equally heated, was then added to them, and the liquid cooled and agitated; it contained 1 grain of the substances in 1550 grains of water previous to dilution for measurement of its energy. The latter then was

Between 30,511 and 33,917 at 14° C. Average 32,214.

Experiment 15.—The diluted constituent solutions of sodium nitrate and sulphuric acid in the proportion of 1 grain of substance in 15,500 grains of water were also chilled to 3° C. and mixed. The energy was then measured, it gave

Between 31,632 and 35,227 at 10° C. Average 33,429.

The difference between the numbers obtained in this case and in Exps. 5, 13, and 14 is not sufficient to prove that the cooling had any real effect upon the amount of energy.

Influence of Light.

Experiment 16.—A solution of the mixture of sodium sulphate and nitric acid was prepared from diluted ingredients which, when added together, produced a liquid containing 1 grain of the mixture in 1550 grains of water. Its voltaic energy was

Between 73,810 and 81,578 at 18° C. Average 77,694.

After standing the liquid in the dark during five days, its voltaic energy was

Between 57,407 and 63,786 at 12° C. Average 60,596 (see also "Influence of Time").

A second portion of the same prepared solution was exposed in a colourless glass bottle to diffused daylight during the same period. Its voltaic energy then was

Between 58,270 and 64,583 at 12° C. Average 61,426.

A third portion, the energy of which at the outset was

Between 69,160 and 76,001 at 25° C. Average 72,598, was exposed in a similar bottle to direct sunlight during five days; its energy was then reduced to

Between 43,055 and 46,407 at 20° C. Average 44,721.

In these experiments daylight had much less effect than sunlight, probably in consequence of the higher temperature in the latter case.

Influence of Magneto-Electric Induction.

Experiment 17.—A portion of the original solution "A" possessing an average voltaic energy = 75,860 at 23°·5 C. was placed in an annular glass vessel surrounding a voltaic coil, and a strong and rapidly intermittent current from two large Grove's elements passed through the coil during one hour, and the voltaic energy again measured; it was

Between 57,407 and 63,786 at 26° C. Average 60,596.

To ascertain whether the change was due to rise of temperature from the heat of the coil, a similar portion of the original liquid was kept during one hour at the same average temperature; its average voltaic energy had then been similarly reduced to 60,596. Magneto-electric induction therefore had no manifest effect.

Behaviour of a Solution of Sodium Sulphate.

Experiment 18.—Dry crystals of the hydrated salt were dissolved in cold water, and the voltaic energy of the solution measured ; it was

Between 1,414 and 2,126 at 9° C. Average 2019 (see also Table II.).

The solution was now heated to about 100° C. in a closed glass flask during two minutes and cooled ; its amount of energy now was

Between 1,839 and 2,039 at 9° C. Average 1939.

And after boiling the solution to dryness and redissolving the salt, the average amount of energy at 9° C. was still = 1,939.

General Conclusions and Remarks.

It is evident from the results obtained that the chemical and molecular constitution of the liquid "A," and the distribution of acids and base in it, are affected by several circumstances: 1st By the degree of dilution of the ingredients at the moment of mixing. 2nd. The temperature of the ingredients at that moment, or to which the mixture has afterwards been subjected. 3rd. The order in which the ingredients have been added to each other. 4th. The amount of light to which the liquid has been exposed. And, 5th, the period of time which has elapsed since it was made. In addition to this, the liquid "A," even under the circumstances most unfavourable to chemical change, suffers a great and variable amount of such change during the mixing of its ingredients.

With regard to the mixture "B," it is well-known that it suffers chemical change, attended by liberation of nitric acid, during the mixing of its constituents. Thermochemical researches have disclosed that it forms a compound, in which one third of the soda is united to the sulphuric and two thirds to the nitric acid, leaving two thirds of the former acid and one third of the latter in a comparatively free state. The present research proves that its ingredients, at the moment of mixing, at once form a comparatively fixed substance or mixture, unalterable by various circumstances which greatly affect the mixture "A ;" it also shows that the expelled portion of nitric acid probably unites chemically with an equivalent portion or one half of the free sulphuric acid ; and it further proves that under certain circumstances the

mixture "A" is changed into a fixed product, having the same amount of voltaic energy as that produced by the ingredients of "B."

With regard to the statement that "the final division of the base between the two acids is the same, whether the soda were originally present as sulphate or nitrate," the present research indicates that this is only true, provided the mixture of sulphate of sodium and nitric acid has been subjected to such conditions or influences, either during or after its preparation, as decompose and convert it into the same product as that of a mixture of nitrate of sodium and sulphuric acid.

Whilst there is greater voltaic energy in the mixture "A," there is more molecular momentum in the one "B;" and whilst the chemical change in "A" may be retarded by low temperature or dilution, it cannot be much prevented in "B" by either of these causes or by both combined. In "A" the chemical change which occurs during mixing only proceeds to a certain stage, if suitable precautions are taken; in "B" it proceeds its entire course, and apparently with greater velocity.

The fact that by using weaker and colder solutions of the separate ingredients of the mixture "A," a larger amount of voltaic energy in the product is obtained, proves that the amount of chemical change which occurs during mixing is a variable quantity, and suggests that it may be still further reduced. The larger the amount of voltaic energy of the freshly-made mixture, the smaller is the amount of chemical change which has occurred during the mixing. As the freshly-made mixture gradually loses voltaic energy at 20° C., the nitric acid gradually expels sulphuric, the proportion of nitrate of sodium increases, and that of sulphate decreases.

Loss of voltaic energy does not always coincide with loss of thermal energy; for instance, in making the mixture "A" heat is absorbed, but in making the one "B" heat is evolved; whilst in both cases the amount of voltaic energy is diminished. In making "A" the loss of energy is 94.86 per cent., and in making "B" 97.70 per cent. (see Table X.).

The "voltaic balance" is a very convenient instrument for detecting and measuring molecular changes in dissolved chemical compounds.

XXXVIII. *On the Ratio of the Electrostatic to the Electromagnetic Units of Electricity.* By HENRY A. ROWLAND, with the assistance of E. H. HALL and L. B. FLETCHER*.

THE determination described below was made in the Laboratory of the Johns Hopkins University about ten years ago, and was laid aside for further experiment before publication. The time never arrived to complete it, and I now seize the opportunity of the publication of a determination of the ratio by Mr. Rosa, in which the same standard condenser was used, to publish it. Mr. Rosa has used the method of getting the ratio in terms of a resistance. Ten years ago the absolute resistance of a wire was a very uncertain quantity, and therefore I adopted the method of measuring a quantity of electricity electrostatically, and then, by passing it through a galvanometer, measuring it electromagnetically.

The method consisted, then, in charging a standard condenser, whose geometrical form was accurately known, to a given potential as measured by a very accurate absolute electrometer, and then passing it through a galvanometer whose constant was accurately known and measuring the swing of the needle.

Description of Instruments.

Electrometer.—This was a very fine instrument, made partly according to my design by Edelmann of Munich. As first made it had many faults which were, however, corrected here. It is on Thomson's guard-ring principle, with the movable plate attached to the arm of a balance and capable of accurate adjustment. The disk is 10·18 centim. diameter in an opening of 10·38 centim., and the guard-plates about 330 centim. diameter. All the surfaces are nickel-plated and ground and polished to optical surfaces and capable of accurate adjustment, so that the distance between the plates can be very accurately determined. The balance is sensitive to a millig. or less, and the exact position of the beam is read by a hair moving before a scale and observed by a lens in the manner of Sir W. Thomson.

The instrument has been tested throughout its entire range by varying the distances and weights to give the constant potential of a standard gauge, and found to give relative readings to about 1 in 400 at least. It is constructed throughout in the most elaborate and careful manner, and the

* Communicated by the Author.

working parts are enclosed in sheet brass to prevent exterior action.

As the balance cannot be in equilibrium by combined weights and electrostatic forces, it was found best to limit its swing to a $\frac{1}{10}$ millim. on each side of its normal position. The mean of two readings of the distance, one to make the hair jump up and the other down, constituted one reading of the instrument.

The adjustments of the plates parallel to each other, and of the movable plate in the plane of the guard-ring, could be made to almost $\frac{1}{40}$ millim.

The formula for the difference of potential of the two plates is

$$V^2 = \frac{8\pi d^2 wg}{A},$$

where d is the distance of the plates, wg the absolute force on the movable plate, and A its corrected area. According to Maxwell

$$A = \frac{1}{2}\pi \left\{ R^2 + R'^2 - (R'^2 - R^2) \frac{\alpha}{d + \alpha} \right\},$$

where R and R' are the radii of the disk and the opening for it, and $\alpha = .221 (R' - R)$. The last correction is only about 1 in 500, and hence we have finally

$$V = 17,221d \sqrt{w} \left[1 + \frac{.0002}{d} \right].$$

Standard Condenser.—This very accurate instrument was made from my designs by Mr. Granow, then of New York, and consisted of one hollow ball, very accurately turned and nickel-plated, in which two balls of different sizes could be hung by a silk cord. The balls could be very accurately adjusted in the centre of the hollow one. Contact was made by two wires about $\frac{1}{100}$ inch diameter, one of which was protruded through the outer ball until it touched the inner one; by a suitable mechanism it was then withdrawn and the second one introduced at another place to effect the discharge. This could be effected five times every second. The diameter of the balls has been accurately determined by weighing in water, and the electrostatic capacities found to be

50.069 and 29.556 C.G.S. units.

A further description is given in Mr. Rosa's paper.

Galvanometer for Electrical Discharges.—This was very carefully insulated by paper, and then put in hot wax in a vacuum to extract the moisture and fill the spaces with wax.

It had two coils each of about 70 layers of 80 turns each, of No. 36 silk-covered copper wire. They were half again as large as the ordinary coils of a Thomson galvanometer. The two coils were fixed on the two sides of a piece of vulcanite, and the needle was surrounded on all sides by a metal box to protect it from the electrostatic action of the coils. A metal cone was attached to view the mirror through. The insulation was perfect with the quickest discharge.

The constant was determined by comparison with the galvanometer described in the American Journal of Science, vol. xv. p. 334. The constant then given has recently been slightly altered. The values of its constant are:—

By measurement of its coils	1832·24
By comparison with coils of electrodynamometer	1833·67
By comparison with single circle	1832·56

Giving these all equal weights, we have 1832·82 instead of 1833·19 as used before.

The ratio of the new galvanometer constant to this old one was found by two comparisons to be

	10·4167
	10·4115
	<hr/>
Mean . . .	10·4141

Hence we have

$$G = 19037.$$

Electrodynamometer.—This was almost an exact copy of the instrument described in Maxwell's Treatise of Electricity, except on a smaller scale. It was made very accurately of brass, and was able to give very good results when carefully used. The strength of current is given by the formula.

$$S = \frac{c \sqrt{K}}{t} \sqrt{\sin \alpha},$$

where K is the moment of inertia of the suspended coil, t its time of vibration, α the reading of the head, and c a constant depending on the number of coils and their form.

Large Coils.

Total number of windings	240
Depth of groove	·84 centim.
Width of groove	·86 "
Mean radius of coils	13·741 "
Mean distance apart of coils	13·786 "

Suspended Coils.

Total number of windings . . .	126
Depth of groove	·41 centim.
Width of groove	·38 "
Mean radius	2·760 "
Mean distance apart	2·707 "

These data give, by Maxwell's formulæ,

$$c = \cdot 006457.$$

In order to be sure of this constant, I constructed a large tangent galvanometer with a circle 80 centim. diameter, and the earth's magnetism was determined many times by passing the current from the electro-dynamometer through this instrument and also by means of the ordinary method with magnets. In this way the following values were found :—

	Magnetic Method.	Electrical Method.
Dec. 16th, 1879	·19921	·19934
Jan. 3rd "	·19940	·19942
Feb. 25th "	·19887	·19948
" 28th "	·19903	·19910
March 1st "	·19912	·19928
	<hr/>	<hr/>
Mean . . .	·19912	·19933

which differ only about 1 in 1000 from each other. Hence we have for c

From calculation from coils . . .	·006457
From tangent galvanometer . . .	·006451

$$\text{Mean . . . } \cdot 006454 \text{ C.G.S. units.}$$

The suspension was bifilar, and no correction was found necessary for the torsion of the wire at the small angles used.

The method adopted for determining the moment of inertia of the suspended coil was that of passing a tube through its centre and placing weights at different distances along it. In this way was found

$$K = 826\cdot6 \text{ C.G.S. units.}$$

The use of the electro-dynamometer in the experiment was to determine the horizontal intensity of the earth's magnetism at any instant in the position of the ballistic galvanometer. This method was necessary on account of the rapid changes

of this quantity in an ordinary building*, and also because a damping magnet, reducing the earth's field to about one third its normal value, was used. For this purpose the ballistic galvanometer was set up inside the large circle of 80 centim. diameter with one turn of wire, and simultaneous readings of the electro-dynamometer and needle of ballistic galvanometer were made.

Theory of Experiment.

We have for the potential

$$V = ed \sqrt{w} \left[1 + \frac{.0002}{d} \right].$$

For the magnetic intensity acting on the needle,

$$H = \frac{2\pi n r^2 c \sqrt{K} \sqrt{\sin \alpha}}{t(r^2 + b^2)^{\frac{3}{2}} \tan \phi}.$$

For the condenser charge,

$$Q = 2 \frac{HT}{\pi G} \sin \frac{\theta}{2} (1 + \frac{1}{2}\lambda) = N \frac{VC}{v}.$$

Whence

$$v = \frac{eGC'r^2}{2nc \sqrt{K}(r^2 + b^2)^{\frac{3}{2}}} \frac{Nt \sqrt{wd}}{T \sqrt{\sin \alpha}} \frac{\tan \phi}{2 \sin \frac{1}{2} \theta} \left[1 - \frac{\lambda}{2} + \&c. \right];$$

but

$$\tan \phi = \frac{1}{2} \frac{\beta}{D} \left[1 - \frac{1}{4} \left(\frac{\beta}{D} \right)^2 + \frac{1}{8} \left(\frac{\beta}{D} \right)^4 \right]$$

and

$$2 \sin \frac{1}{2} \theta = \frac{1}{2} \frac{\delta}{D} \left[1 - \frac{1}{3} \left(\frac{\delta}{D} \right)^2 \right] \text{ nearly.}$$

So that, finally,

$$v = \frac{eGC'r^2}{2nc \sqrt{K}(r^2 + b^2)^{\frac{3}{2}}} \frac{Nt\beta \sqrt{w}}{T \sqrt{\sin \alpha}} \frac{d}{\delta} [1 - A - B - C + D + E - F + I].$$

A = 0 ; .0011 ; .0030 ; .0056 ; .0090 for 1, 2, 3, 4, 5 discharges as investigated below.

$$B = \frac{1}{4} \left(\frac{\beta}{D} \right)^2 - \frac{1}{8} \left(\frac{\beta}{D} \right)^4.$$

* This experiment was completed before the new physical laboratory was finished.

$$C = \frac{\lambda}{2}.$$

$$D = \frac{.0002}{d}.$$

$$E = \frac{1}{3} \left(\frac{\delta}{D} \right)^2.$$

$F = .0013$ for first ball of condenser and $.0008$ for other, as investigated below.

$I =$ correction for torsion of fibre $= 0$, as it is eliminated.

$e =$ constant of electrometer $= 17.221$.

$G =$ „ ballistic galvanometer $= 19087$.

$r =$ radius of large circle $= 42.105$ centim.

$n =$ number of coils on circle $= 1$.

$c =$ constant of electro-dynamometer $= .006454$.

$K =$ moment of inertia of coil of electro-dynamometer $= 826.6$.

$b =$ distance of plane of large circle from needle $= 1.27$.

$C' =$ capacity of condenser $= 50.069$ or 29.556 .

$D =$ distance of mirror from scale $= 170.18$ centim.

$w =$ weight in pan of balance.

$t =$ time of vibration of suspended coil.

$T =$ „ „ needle of ballistic galvanometer.

$\beta =$ deflexion of needle on scale when constant current is passed.

$\delta =$ swing caused by discharge of condenser.

$d =$ distance of plates of electrometer.

$N =$ number of discharges from condenser.

$\lambda =$ logarithmic decrement of needle.

$A =$ correction due to discharges not taking place in an instant.

$\alpha =$ reading of head of electro-dynamometer when constant current is passed.

The principal correction requiring investigation is A . Let the position and velocity of the needle be represented by

$$x = a_0 \sin bt \quad \text{and} \quad v = a_0 b \cos bt, \quad \text{where } b = \frac{\pi}{T}.$$

At equal periods of time, $t_1, 2t_1, 3t_1$, &c. let new impulses be given to the needle so that the velocity is increased by v at each of these times. The equations which will represent the position and velocity of the needle at any times are, then,

between 0 and t_1 , $x = a_0 \sin bt$; $v = a_0 b \cos bt$;

„ t_1 and $2t_1$, $x = a' \sin b(t + t')$; $v = a' b \cos b(t + t')$;

„ $2t_1$ and $3t_1$, $x = a'' \sin b(t + t'')$; $v = a'' b \cos b(t + t'')$;

&c. &c. &c.

At the time 0, t_1 , $2t_1$, &c. we must have :—

$$x = 0; \quad v_0 = a_0 b;$$

$$a_0 \sin bt_1 = a' \sin b(t_1 + t'); \quad v_0 + a_0 b \cos bt_1 = a' b \cos b(t_1 + t');$$

$$a' \sin b(2t_1 + t') = a'' \sin b(2t_1 + t''); \quad v_0 + a' b \cos b(2t_1 + t') \\ = a'' b \cos b(2t_1 + t'');$$

&c. &c.

Whence we have the following series of equations to determine a' , a'' , &c., and t' , t'' , &c.:—

$$a_0^2 b^2 = v_0^2;$$

$$a'^2 b^2 = a_0^2 b^2 + v_0^2 + 2v_0 a_0 b \cos bt_1; \quad \sin b(t_1 + t') = \frac{a_0}{a'} \sin bt_1;$$

$$a''^2 b^2 = a'^2 b^2 + v_0^2 + 2v_0 a' b \cos b(2t_1 + t'); \\ \sin b(2t_1 + t'') = \frac{a'}{a''} \sin b(2t_1 + t');$$

$$a'''^2 b^2 = a''^2 b^2 + v_0^2 + 2v_0 a'' b \cos b(3t_1 + t'''); \\ \sin b(3t_1 + t''') = \frac{a''}{a'''} \sin b(3t_1 + t'');$$

&c. &c.

When t_1 is small compared with the time of vibration of the magnet, we have very nearly

$$t' = -\frac{1}{2} t_1, \quad t'' = -t_1, \quad t''' = -\frac{3}{2} t_1, \text{ &c.}$$

$$a'^2 = 2a_0^2(1 + \cos bt_1) = 4a_0^2(1 - \frac{1}{4}(bt_1)^2),$$

$$a''^2 = 9a_0^2(1 - \frac{2}{3}(bt_1)^2),$$

$$a'''^2 = 16a_0^2(1 - \frac{5}{4}(bt_1)^2),$$

$$a''''^2 = 25a_0^2(1 - 2(bt_1)^2).$$

Whence

$$\begin{aligned} a' &= 2a_0 \left(1 - \frac{1}{8} (bt_1)^2\right). \\ a'' &= 3a_0 \left(1 - \frac{1}{3} (bt_1)^2\right). \\ a''' &= 4a_0 \left(1 - \frac{5}{8} (bt_1)^2\right). \\ a'''' &= 5a_0 \left(1 - (bt_1)^2\right). \end{aligned}$$

Now a_0, a', a'', a''' , and a'''' are the values of δ with 1, 2, 3, 4, and 5 discharges, and $a_0, 2a_0, 3a_0, 4a_0$, and $5a_0$ are the values provided the discharges be simultaneous.

Hence the correction, A , has the values $0, \frac{1}{8}(bt_1)^2, \frac{1}{3}(bt_1)^2, \frac{5}{8}(bt_1)^2$, and $(bt_1)^2$ with 1, 2, 3, 4, and 5 discharges. The value of t_1 is about one fifth of a second, and hence $(bt_1)^2 = \cdot 009$ nearly. The values of A are then 0, $\cdot 0011, \cdot 0030, \cdot 0056$, and $\cdot 0090$.

This correction is quite uncertain as the time t_1 is uncertain.

In assuming that the impulses were equal, we have not taken account of the angle at which the needle stands at the second and subsequent discharges, nor the magnetism induced in the needle under the same circumstances. One would diminish and the other increase the effect. I satisfied myself by suitable experiments that the error from this cause might be neglected.

The method of experiment was as follows:—The store of electricity was contained in a large battery of Leyden jars. This was attached to the electrometer. The reading of the potential was taken, the handle of the discharger was turned, and the momentary swing observed and the potential again measured. The mean of the potentials observed, with a slight correction, was taken as the potential during the time of discharge. This correction came from the fact that the first reading was taken before the connexion with the condenser was made. The first reading is thus too high by the ratio of the capacities of the condenser and battery and the mean reading by half as much. Hence we must multiply d by $1 - F$, where $F = \cdot 0013$ for first ball of condenser and $\cdot 0008$ for the other. This will be the same for 1 or 5 discharges.

From ten to twenty observations of this sort constituted a set; and the mean value of $\frac{d}{8}$, which was calculated for each observation separately, was taken as the result of the series.

Before and after each series the times of vibration, t and T , and the readings, β and α , were taken. The logarithmic decrement was observed almost daily.

	January 15, 1879.			January 17.			
	10	10	18	10	10	10	16
C ...	50·069	50·069	50·069	50·069	50·069	50·069	50·069
ω ...	2	2	2	2	2	2	2
t ...	2·436	2·436	2·436	2·436	2·436	2·436	2·436
β ...	34·03	34·08	33·82	33·10	33·10	32·99	32·99
$\frac{d}{\delta}$...	·4839	·2400	·4851	·4880	·1624	·0981	·4900
N ...	1	2	1	1	3	5	1
D ...							
λ ...	·03583	·03583	·03583	·03424	·03424	·03424	·03424
T ...	6·7505	6·7505	6·7467	6·636	6·636	6·631	6·631
α ...	14° 56' 17"	14° 56' 17"	14° 53' 5"	15° 18' 50"	15° 18' 50"	15° 14' 48"	15° 14' 48"
δ^* ...	4·2	7·2	4·2	4·3	11·5	18·3	3·7
$v \times 10^{-8}$	300·59	298·37	300·17	296·72	295·73	296·50	297·84

	February 4.		February 6.		February 7.	
	20	20	18	18	19	19
C	29·556	29·556	29·556	29·556	29·556	29·556
ω	3	3	4	4	4	4
t	2·436	2·436	2·436	2·436	2·436	2·436
β	33·08	32·72	33·19	33·18	32·27	32·44
$\frac{d}{\delta}$	·17450	·69525	·29572	·58823	·11986	·19938
N	4	1	2	1	5	3
D						
λ	·03500	·03500	·03500	·03500	·0352	·0352
T	6·822	6·825	6·811	6·828	6·809	6·809
α	14° 47' 25"	14° 35' 45"	14° 51' 0"	14° 43' 40"	14° 11' 20"	14° 20' 20"
δ^* ...	7·5	2·0	3·3	1·8	7·3	4·7
$v \times 10^{-8}$	301·82	300·80	297·43	296·56	297·38	298·75

* Approximate value

the results of all the observations.

January 20.		January 22.		January 24.		Jan. 27.	Feb. 3.
18	18	18	18	18	18	18	20
50·069	50·069	50·069	50·069	50·069	50·069	50·069	29·556
2	2	2	2	3	3	3	3
2·435	2·435	2·437	2·437	2·436	2·436	2·437	2·435
33·43	33·18	33·60	33·60	34·53	34·30	33·64	33·79
·4871	·09759	·15954	·48065	·19588	·39279	·09777	·17145
1	5	3	1	2	1	4	4
·0350	·0350	·03578	·03578	·03507	·03507	·03507	·03500
6·693	6·689	6·792	6·783	6·796	6·788	6·7944	6·8471
$15^{\circ} 2' 35''$	$14^{\circ} 57' 45''$	$14^{\circ} 25' 30''$	$14^{\circ} 25' 52''$	$15^{\circ} 6' 25''$	$14^{\circ} 59' 40''$	$14^{\circ} 25' 22''$	$15^{\circ} 14' 10''$
4·1	16·8	10·8	3·7	7·7	4·0	13·3	8·0
298·90	296·37	296·40	298·57	298·61	299·05	296·43	297·24

February 11.		February 12.		February 14.		February 17.	
18	18	18	18	18	18	18	18
29·556	29·556	29·556	29·556	29·556	29·556	29·556	29·556
2	2	2	2	1	1	1	1
2·436	2·436	2·436	2·436	2·435	2·435	2·436	2·436
32·89	32·75	32·82	32·42	32·72	32·39	31·77	31·39
·417	·16744	·16767	·42264	·39752	·29647	·40215	·30109
2	5	5	2	3	4	3	4
·0356	·0356	·0354	·0354	·0361	·0361	·0348	·0348
6·8734	6·8557	6·860	6·854	6·890	6·890	6·788	6·778
$14^{\circ} 13' 35''$	$14^{\circ} 7' 15''$	$14^{\circ} 16' 10''$	$14^{\circ} 1' 10''$	$14^{\circ} 8' 0''$	$13^{\circ} 59' 0''$	$14^{\circ} 17' 30''$	$14^{\circ} 1' 0''$
4·3	9·3	10·3	4·3	6·2	9·5	8·6	9·5
297·78	296·87	296·31	300·19	298·66	295·02	296·75	295·22

for correction only.

These results can be separated according to the number of discharges as follows:—

1.	2.	3.	4.	5.
300.59	298.37	295.73	296.43	296.50
300.17	298.61	296.40	297.24	296.37
296.72	297.43	298.75	301.82	297.38
297.84	297.78	298.66	295.02	296.87
298.90	300.19	296.75	295.22	296.31
298.57				
299.05				
300.80				
296.56				
298.80	298.48	297.26	297.15	296.69

In taking the mean I have ignored the difference in the weights due to the number of observations, as the other errors are so much greater than those due to estimating the swing of the needle incorrectly.

It will be seen that the series with one discharge is somewhat greater than those with a larger number. This may arise from the uncertainty of the correction for the greater number of discharges, and I think it is best to weight them inversely as this number. As the first series has also nearly twice the number of any other, I have weighted them as follows:—

Weight.	$v \times 10^{-8}$.
8	298.80
4	298.48
3	297.26
2	297.15
1	296.69
Mean	298.15

Or $v=29,815,000,000$ cm. per second.

It is impossible to estimate the weight of this determination. It is slightly smaller than the velocity of light, but still so near to it that the difference may well be due to errors of experiment.

Indeed, the difference amounts to a little more than half of one per cent.

It is seen that there is a systematic falling-off in the value of the ratio. This is the reason of my delaying the publication for ten years.

Had the correction, A, for the number of discharges been omitted, this difference would have vanished; but the correction seems perfectly certain, and I see no cause for

omitting it. Indeed I have failed to find any sufficient cause for this peculiarity, which may, after all, be accidental.

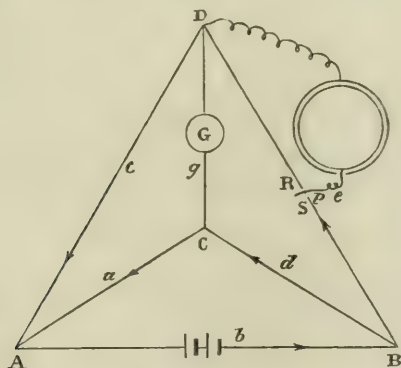
As one of the most accurate determinations by the direct method, and made with very elaborate apparatus, I think, however, it may possess some interest for the scientific world.

XXXIX. *Determination of v , the Ratio of the Electromagnetic to the Electrostatic Unit.* By EDWARD B. ROSA, *Student in Physics in the Johns Hopkins University*.*

THIS investigation was conducted in the Physical Laboratory of Johns Hopkins University during the months of March to June 1889, under the direction of Associate Professor A. L. Kimball. The writer takes great pleasure in acknowledging his obligations to Dr. Kimball for valuable advice and encouragement throughout the progress of the work.

The method employed is essentially that given by Maxwell, vol. ii. § 776. It was used by J. J. Thomson in his determination of v , published in the 'Philosophical Transactions' for 1883. The following is substantially his description of the method. In a Wheatstone bridge, A B C D (fig. 1), the circuit

Fig. 1.



BD is not closed, but the points D and B are joined to two poles R and S of a commutator, between which vibrates the armature p , which is connected with the inner shell of a spherical condenser. When p touches S the condenser will be charged, and there will be a momentary current through

* Communicated by Prof. Rowland.

the various arms of the bridge, through the galvanometer from D to C. When p touches R the two surfaces of the condenser are connected, and the latter discharges itself through D R. If now the armature be made to vibrate continuously there will be a series of momentary currents through the galvanometer, and by adjusting the resistance a (c and d being large, fixed resistances), these interrupted currents may be exactly counterbalanced by the steady current from C to D, and the resultant deflexion of the galvanometer is zero. When this is the case there is a relation between the capacity of the condenser, the number of times the latter is charged and discharged per second, and the resistances in the various arms of the bridge. Maxwell gives an approximate value of this relation. Thomson's more complete investigation gives the following equation :—

$$nC = \frac{a \left\{ 1 - \frac{a^2}{(a+c+g)(a+b+d)} \right\}}{cd \left\{ 1 + \frac{ab}{c(a+b+d)} \right\} \left\{ 1 + \frac{ag}{d(a+c+g)} \right\}}$$

where n is the number of complete oscillations of the armature p per second; C is the capacity of the condenser in electro-magnetic measure; and the other letters the resistances of the various arms of the bridge, as shown in fig. 1. In the present case the values of these resistances were about as follows:—

$$\begin{array}{ll} a = 40 \text{ to } 1900 \text{ ohms.} & d = 100,000 \text{ ohms.} \\ b = 0 \text{ nearly.} & g = 6,000 \text{ ,,} \\ c = 1,570,000 \text{ to } 2,450,000 \text{ ohms.} & \end{array}$$

Owing to the very high values of c and d as compared with a , b , and g , the above equation may be replaced by the approximate one, $C = \frac{a}{ncd}$, which is true to within a hundredth of one per cent. The electrostatic capacity, C' , is determined by calculation from the geometrical constants of the condenser. The ratio of these values of the capacities, $\frac{C'}{C}$, is v^2 , the square root of which, v , is the quantity sought.

Advantages of the Method.

Thus there appears at once an important advantage of the method of determining the ratio of the units from the values of a capacity, namely, that v is the square root of the ratio of

the capacities, and any error in the latter enters into v by only half its amount.

There are several important advantages of this method of measuring the electromagnetic capacity. In the first place a knowledge of the exact electromotive force and resistance of the battery is not required, and their constancy is not essential. In the second place, since it is a null method, such uncertain quantities as logarithmic decrement, torsion of the suspending fibre, and period of the needle are not required; the galvanometer can readily be made more sensitive than a ballistic galvanometer; its "constant" need not be known; and the field of force may be variable both in intensity and direction without prejudice to the experiment. On the other hand, the quantities which are required are the period of the vibrator and the values of three resistances, quantities which are capable of determination to a very high degree of accuracy. In the present case the vibrator was either a tuning-fork or else it was driven by a tuning-fork, and by the arrangement adopted the uncertainty in its period was reduced to an extremely small quantity. The difficulties and limits of the method will appear under the head of Sources of Error.

Instruments.

1. *Condenser*.—This was made from designs by Prof. Rowland. It consists of a hollow sphere whose radius is 12·7 centim., and within which may be hung either of two balls of 10·1 and 8·9 centim. radius respectively. The condenser has a capacity of about 50 absolute electrostatic units with the larger ball and 30 with the smaller.

The spherical surfaces are accurately ground, nickel-plated and polished to a mirror surface. The ball is suspended by a silk cord C (fig. 3) passing through a hole 7 millim. in diameter in the outer shell, and attached to the insulated end of a pivoted beam and counterpoised. By means of a rack and pinion movement and vernier, the ball may be accurately set in any desired position. Maxwell* objects to this form of condenser on account of the difficulty of working the surfaces accurately spherical, making them truly concentric, and determining with sufficient accuracy their dimensions. That these difficulties have in the present case been entirely surmounted will, I think, appear from the discussion under the heads of Displacement of the Ball (p. 323) and Electrostatic Capacity (p. 323).

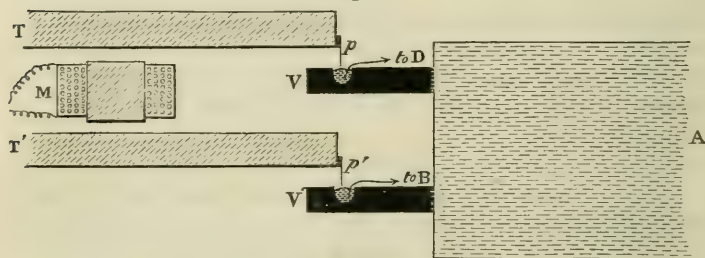
* Vol. i. p. 321.

2. *Galvanometer*.—This was one of Elliott Bros.' Thomson high-resistance, astatic galvanometers, made very sensitive.

3. *Tuning-Forks*.—Two of Koenig's forks were used, whose frequencies were approximately 32 and 130 per second. They were driven by three or four Bunsen cells, the same current in the case of the slower fork operating the vibrator p (fig. 1). Their exact periods were determined by Michelson's method*.

4. *Vibrators*.—The oscillating piece p in the case of the slower fork was a commutator such as that used by Thomson†. The action of this form of vibrator was regular and satisfactory in the case of the slower fork; but with the higher fork great difficulty was experienced in obtaining sufficient uniformity, and finally it was abandoned and the following plan devised as a substitute. T, T' (figs. 2 and 3) are two

Fig. 2.



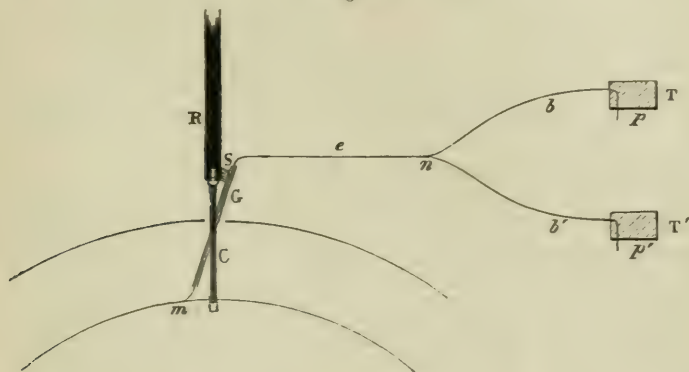
prongs of the tuning-fork, driven by the electromagnet M ; the interrupter, attached to the end of one of the prongs, not being shown. b, b' (fig. 3) are two fine brass wires, uniting at n and tipped with platinum at p, p' , where they are bent at right angles and fastened to the fork with an insulating cement. V, V' are two small blocks of vulcanite attached to a firm support A . Below the platinum points are two cavities in the vulcanite which are filled with mercury, and as the fork vibrates first one and then the other of the points dips into the mercury. Thus the mercury cups, which are joined to B and D respectively (fig. 1) answer to the posts S and R , while the wires b, b' unite and, passing through the fine glass tube G , reach the ball of the condenser at m . When the prongs separate, p' dips into the lower cup and the

* Phil. Mag. [5] xv. p. 84 (1883).

† Thomson, Phil. Trans. 1883, or Glazebrook, Phil. Mag. [5] xviii. p. 98.

condenser is charged; when they approach, p dips into the other cup and the condenser is discharged. The points must be at least half a millimetre above the surfaces of the mercury when the fork is at rest, in order to avoid

Fig. 3.



both dipping at once and short-circuiting the condenser. With an amplitude of about three millimetres perfect contact is made at each vibration, and the regularity of action, as shown by the steadiness of the spot of light on the scale, is extremely satisfactory. The deflexion of the needle when the steady current is not balanced by the intermittent current amounts in the case of the high fork to 125 scale-divisions using the one-tenth shunt; or, without the shunt, as it was used in practice, to 1250 scale-divisions. With its best action the resistances were adjusted until closing the key would cause a deflexion of less than half a scale-division, corresponding to less than 1-2500th part of the whole current. To obtain a regularity of action which permitted such accurate observations required a very delicate adjustment of the distances between the surfaces of the mercury and the points above them, as well as clean surfaces and a steady current.

5. *Battery*.—About forty cells of a storage battery, with a total electromotive force of about eighty volts, were used. A higher electromotive force, at first proposed, was thought to be unnecessary.

6. *Resistances*.—The resistance a was taken from a box of Elliott Bros., the total resistance of which was about 12,000 ohms; the resistance d was a 100,000 ohm box from the same firm. The first of these, box A, was carefully calibrated by

comparing the several coils on a Fleming bridge with three standard coils of 10, 100, and 1000 ohms respectively. The first was a Warden-Muirhead No. 292, 10 B. A. U. Its value, determined by Glazebrook, Oct. 1887, is 9.99416 at $16^{\circ}.5$ C. The other two had been previously carefully compared with this. The values of the resistances of box A adopted were the means of three different and closely agreeing determinations, made at different temperatures. The several coils of box B were carefully compared with the known resistances of A. The temperature-coefficients of both boxes were also carefully determined.

The resistance c was of graphite. Plate-glass was ground with fine emery and lines ruled upon it. Under a magnifying power of several hundred diameters the layer of graphite appears made up of patches which run together at numerous points. The resistance of a strip of graphite of given length and breadth depends upon how well these patches are joined together. The glass and graphite are given a heavy coat of shellac and thoroughly dried. A series of ten such resistances were prepared and mounted, connexion being made at the ends by tin-foil, held firmly in contact with the graphite by rubber packing, wires passing out from the tin-foil. The resistances were placed in cylindrical boxes with vulcanite tops, in which were set binding-screws, joined to the wire terminals. The boxes can be surrounded by water or other material to lessen the temperature fluctuations. These resistances proved quite constant and reliable. Two were used in this experiment, R_2 and R_3 , whose resistances were approximately 1,570,000 and 2,450,000 ohms. During the six weeks preceding May 9, their alteration, aside from temperature fluctuations, was inappreciable. But between May 9 and May 13, when not in use, from some as yet unknown cause, both increased about one half of one per cent., and up to June 8, when last used, remained nearly constant at the new value. Inasmuch as glass and shellac are poor conductors, the temperature of the graphite resistances cannot safely be assumed to be the same as that of the air within the box, unless the latter has been kept constant for some time. In order, therefore, to avoid all uncertainty as to their values these resistances were determined anew whenever used; and if their temperature changed materially, both just before and just after using. They were compared with the resistances of boxes A and B, two arms of a Wheatstone bridge, with a ratio of 99.89, being taken from A. Here is a specimen observation and calculation:—

May 22. Bridge reading 24,430. Temperatures : Graphite $19^{\circ}\cdot 8$, $A = 20^{\circ}\cdot 3$, $B = 20^{\circ}\cdot 6$.

$$24,430 = \begin{cases} 20000 \text{ from } B = 20,012 \text{ at } 20^{\circ} = 20,015 \text{ at } 20^{\circ}\cdot 6 \\ 4430 \quad ,, \quad A = 4443 \quad ,, \quad = 4444 \quad ,, \quad 20^{\circ}\cdot 3 \end{cases}$$

$$24,459$$

$24,459 \times 99\cdot 89 = 2,443,200$ ohms at $19^{\circ}\cdot 8 = \text{temp. at which used.}$

This value is reliable to within one part in five thousand.

It is proper to add that if these graphite resistances are put into a circuit where there is a large difference of potential between their terminals, their resistance is immediately diminished by heating. With three Bunsen cells used in measuring their resistance no heating was perceptible. In the determination of capacity there was a difference of potential between the terminals always less than two volts, and usually less than one. When the temperatures were maintained constant, the resistance after use was always precisely the same as before. While, therefore, the use of high graphite resistances is somewhat restricted where great accuracy is desired, they still may serve a very useful purpose in many cases, and are the most convenient and reliable of any high resistance, aside from metal wires, that I know anything about.

Arrangement of the Apparatus.

The vibrators were fixed as near as possible to the condenser to reduce the capacity of the charging wires to a minimum. The condenser, galvanometer, and other parts of the apparatus were insulated with great care, and yet in spite of all precautions leakage made its appearance on rainy days, and a slight trace of leakage could usually be detected. Observations were consequently confined to fair weather. The apparatus for the determination of the frequency of the forks was always ready for use.

Sources of Error.

1. *Resistances.*—The constant errors in the resistances must have been very small, and corrections were always carefully made for temperature-fluctuations.

2. *Tuning-Forks.*—Michelson's method furnishes a very exact determination of the period of an electric tuning-fork ; but unfortunately the period does not remain constant. This is especially the case with the higher fork, the charging wires

and spring-contact having a varying effect upon the rate in different adjustments. But the slower fork with mercury contact was not, even after making proper temperature-corrections, perfectly constant. To avoid all uncertainty, and obviate the necessity of applying a temperature-correction, the rates of the forks were determined each time anew, usually before and after or in the midst of a series of observations on capacity. As stated, the apparatus for the purpose was always ready for use; and without stopping the fork or changing its circumstances in any way whatever, by simply closing the clock-circuit and the primary circuit of the induction-coil, I could in three to five minutes count a sufficient number of flashes to give me the period of the fork true to within less than one part in ten thousand. Occasionally a slight change in the sound emitted by the fork, due to variation in contact or current, suggested a possible change in the period; a moment's glance in the microscope would answer the question. This method of dealing with the rates of the forks avoids the introduction of small constant and large accidental errors, which may happen when the rates are determined once for all.

3. *Charging-Wires*.—The vibrating armature p , the wires b, b' , as well as the joining-wire e , have a certain capacity, which adds itself to that of the condenser when they are connected, but which may be determined separately by disconnecting the charging-wire at m . Thus, on April 15, with R_5 and fast fork, the resistance a was 1874.5 and 153 respectively in the two cases mentioned, which gives 1721.5 as the resistance corresponding to the condenser alone. This assumes that the capacity of the charging-wire is the same when joined to the ball as when separated. The capacity of the two and a half centimetres of fine brass wire between the ball and the shell (fig. 3) is nearly one per cent. of the capacity of the condenser, determined experimentally. It would seem that this capacity might be slightly greater when the wire was disconnected from the ball and at a different potential; but being lifted one or two millimetres in disconnecting, its capacity would be thereby reduced. The effects of these two modifying circumstances were separately very carefully studied. With the rapid fork running very smoothly, a change of half an ohm could be easily detected; this would be equal to a change of $\frac{1}{3506}$ of the capacity of the condenser. No difference, however, could be observed, although the trial was several times repeated. The two effects have opposite signs; and if each is inappreciable, much more would their sum be

so. I therefore conclude that the difference between the observed capacities of condenser and charging system together and of charging system alone is a true measure of the capacity of the condenser.

4. *Displacement of the Ball.*—The upper half of the spherical shell was lifted and the lower half adjusted upon its supports until the distance of the ball from the shell was the same at all points on the equatorial circumference. The upper half of the shell was then replaced, and by means of the rack and pinion the ball was first lowered and then raised until it touched the shell, the exact moment of touching being indicated by an electrical contact, and several readings taken on the vernier in each position. The mean of the readings in the two positions gave the central position. In this manner the ball was adjusted vertically to within 0.1 mm., and equatorially within 0.2 mm. Thus the ball is centred to within less than one per cent. of the distance between the ball and shell, which is 25 mm. Thomson has investigated a formula for the capacity of eccentric cylinders. The formula shows that for a displacement of one per cent. the capacity is increased $\frac{1}{200}$ of one per cent. Evidently the capacity of spherical shells is less affected by slight eccentricity than that of cylinders. Therefore we may safely conclude that no error is due to eccentricity. This conclusion was verified experimentally, a displacement of four per cent. causing an inappreciable change.

5. *Adjusting Resistances.*—The accidental errors occurring in adjusting the resistance a so as to produce zero deflexion will be eliminated by a large number of observations. Their magnitude depends on the strength of the current, delicacy of the galvanometer, regularity of the vibrator, &c., and is larger with the slow fork than with the fast. The stronger the current and the more sensitive the galvanometer, the greater the deflexion due to a certain error in the resistance a ; but, on the other hand, the greater the unsteadiness of the spot, so there is a practical limit in that direction. That these accidental errors are small is, I think, attested by the uniformity of the results obtained.

Electrostatic Capacity.

The electrostatic capacity of the condenser was calculated from the formula

$$C = \frac{rr'}{r - r'},$$

where r , r' are the radii of the shell and ball respectively. The radii are determined by finding the volume of water which fills the shell and which is displaced by the ball. These results are confirmed by direct measurement upon the dividing-engine.

Ball A.

May 1.—Weight in air, 2903·83 g. Temperature, $18^{\circ}\cdot 9$; Bar., 76·0 cm. Volume of ball, 4339 c. c. approximately ; volume of brass weights, 340 c. c. approximately. Correction for displaced air is consequently +4·83 g. \therefore weight of ball *in vacuo* = 2908·66 g. A second determination gave 2908·64 g. I therefore take for the true weight *in vacuo*, **2908·65 g.** The ball being lighter than water, a sinker was attached and the following weighings made:—

May 3. Weight in distilled water,	
ball and sinker . . .	210·62 g. at $17^{\circ}\cdot 05$ C.
Ditto, sinker alone . . .	1635·59 g. at $17^{\circ}\cdot 10$ C.
	<hr/>
Difference . . .	1424·97 g.
Correction for $\frac{1425}{8\cdot 4}$ c. c. air	
displaced by weights . .	·21
	<hr/>
	1424·76
Weight of ball <i>in vacuo</i> .	2908·65 g.
	<hr/>
Loss of weight in water at	
$17^{\circ}\cdot 05$ C.	4333·41 g.
Ditto, at 4° C.	4338·68 g. = volume in
	cubic centimetres.

Another determination at a different temperature gave 4338·87. I take as a mean 4338·8, which makes the mean radius $r'_A = 10\cdot 1180$ cm. An error of 0·1 in the number 4338·8 would cause an error of less than a thousandth of a millimetre in r'_A .

Ball B.

May 1. Weight in air . . .	2321.40 g.
Correction for displaced air	3.20 g. = 2324.60 g. in <i>vacuo.</i>
May 3. Weight in distilled water,	
ball and sinker . . .	208.96 g. at 16°·45 C.
Ditto, sinker alone . . .	807.86 g. at 16°·70 C.
Difference . . .	598.90
Correction for air displaced by weights09
	<hr/> 598.81
Weight of ball in <i>vacuo</i> .	2324.60 g.
Loss of weight in water at 16°·45 C.	2923.41
Ditto, at 4° C.	2926.65 = volume in cubic centimetres.

This gives $r'_B = 8.8735$. A second determination gave a closely agreeing result.

In these weighings the bodies were lifted completely out of water, replaced, and air-bubbles carefully removed at least three times in each weighing. The mean of the several values, which differed in the centigrams, was each time taken. These differences were usually due to slight changes in the temperature of the water, the balances being far more sensitive than the thermometer. As, however, the temperature was read to $\frac{1}{10}$ of a degree several times during a weighing, and the mean taken, it is thought that the temperature is true to within 0°·1; and this corresponds in the case of the large ball to about .07 g. I think the values of the radii given above are true to within two or three thousandths of a millimetre.

Shell.

The weighings of water contained by the shell were made by replacement. The shell was sealed about the junction of its two halves with white paint, filled with distilled water, and allowed to stand to absorb any air-bubbles which might have escaped the brushing with a wire which was given the inner surface after filling. The condenser was placed on the platform of the scales, approximately counterpoised, and then accurately balanced by adding weights to the platform; about 100 c. c. of water was then withdrawn, temperature taken, shell refilled (the space around the opening being thoroughly

raked with a wire, to prevent error from small air-bubbles which tended to lodge there), and weights again added to balance. The following weighings were thus made:—

130.7 g.	130.5 g.	131.2 g.	131.3 g.	Mean=131.4.
132.0 g.	132.0 g.	131.7 g.	131.4 g.	Mean temp., 18° 4.
		131.6 g.	131.4 g.	

The condenser being emptied and carefully dried, required the following weights to balance the same counterpoise:—

8650.3 g. 8650.8 g. 8650.9 g. 8650.9 g. Mean 8650.7 g.

$$8650.7 - 131.4 = 8519.3 = \text{weight of water at } 18^{\circ}.4 \text{ in air.}$$
$$8531.8 = \quad \quad \quad 4^{\circ.0} \quad \quad \quad$$

Correction for dis-	} 9.1
placed air . .	

8540·9 = " " " *in vacuo*

= volume of the shell in cubic centimetres. This makes the radius $r = 12.6805$ cm. It seems reasonable to suppose that the number 8540.9 is true to within less than a gram. This would make the error in r less than .0005 cm.

These values of the radii are confirmed by the following direct measurements, made on a dividing-engine, using calipers and a standard metre-bar by Bartels and Diederichs, Göttingen, whose length is accurately known. Three mutually perpendicular diameters of the shell were found to be

25.357, 25.360, 25.358. Mean = 25.3583.

giving $r=12.6791$, a very close agreement in view of the difficulty of setting the calipers. More accurate measurements on the balls were obtained.

Ball A.—Following are twelve diameters:—

20-2399	20-2372	20-2170
20-2358	20-2336	20-2348
20-2350	20-2382	20-2250
20-2250	20-2315	20-2401

Mean = 20.2328, + Correction for the bar .0038, = 20.2366 :

$$\therefore r'_A = 10.1183 \text{ cm.}$$

Ball B.—Following are six diameters:—

17-7468	17-7408	17-7429
17-7465	17-7452	17-7407

Mean = 17.7438, + Correction .0034, = 17.7472 ;

$$\therefore r_B = 8.8736 \text{ cm.}$$

It is perhaps somewhat accidental that these values coincide so closely with the values of the radii found by the first method. Their importance is not insisted upon further than as furnishing satisfactory confirmation of the results of the other and more accurate method.

It will be seen that in ball A no diameter differs from the mean by as much as a tenth of a millimetre, and in B the variation is still smaller. This deviation from perfect sphericity has no appreciable effect upon the value of the capacity calculated from the ordinary formula. We now have

$$C'_A = \frac{12.6805 \times 10.1180}{12.6805 - 10.1180} = 50.069.$$

$$C'_B = \frac{12.6805 \times 8.8735}{12.6805 - 8.8735} = 29.556.$$

The radius of the hole in the shell through which the suspending cord (C, fig. 3) passes is .35 cm., and its area $\frac{1}{5200}$ of the area of the shell. The capacity is diminished in a less ratio than the area; therefore the capacity is diminished probably not more than a hundredth of one per cent.—a quantity wholly negligible.

Electromagnetic Capacity.

A series of observations on the electromagnetic capacity by the method described was made, extending from March 28 to June 8, under a variety of circumstances as to weather and external surroundings. The two graphite resistances, the two tuning-forks, and different resistances from box A were variously combined, and at temperatures varying from 17° to 25° C. The shell and ball were occasionally readjusted, and between April 16 and May 4 the condenser was taken apart and its electrostatic capacity determined. Further, in order to measure the graphite resistances the apparatus as shown in fig. 1 was each time disconnected and put together again. All these variations must have had the effect of eliminating to a large degree constant errors, while of course the single observations do not agree so well among themselves as they otherwise would. Following is the last observation made, given as a specimen:—

$$\begin{aligned} \text{Resistances } a: & \quad (1) 1930.0 - (2) 194.0 = 1736.0 \\ & \quad (3) 1932.0 - (4) 195.5 = 1736.5 \\ & \quad (5) 1932.0 - (6) 197.0 = 1735.0 \\ & \quad (7) 1932.5 - (8) 196.5 = 1736.0 \end{aligned}$$

Temperatures : A = 22°·3, B = 23°·0, Graphite = 23°·0.

The wire was first in contact at m (fig. 3), and the resistance a corresponding to joint capacity of condenser and charging-system was 1930.0 ohms. The wire was now lifted very slightly, and 194.0 ohms found to give no deflexion on closing the key in the galvanometer-circuit. The wire was then lowered to make contact, and the subsequent observations in the order of the numbers made. Any leakage increases the numbers alike in the first two columns, and if constant does not affect the differences, which give the capacity of the condenser. But the leakages are not constant, so that small differences are thereby introduced; this accounts in part for the differences above, though of course small differences are inevitable if there be no leakage. On June 6 the wooden base of the condenser was thoroughly wet with a cloth; and the leakage thereby introduced changed the readings from 1924.0–186.0 to 1933.0–195.0, the difference, 1738.0, remaining unaltered.

The mean of the above differences is 1735.9

Correction: Excess at 20° C., 5.4; temp. corr., +1.3 6.7

$$1742.6 = a.$$

Box B=100,120 at 23° 0 = d .

R_5 (calculated as already explained), 2,435,800 = c .

Frequency of the fork, 130.075 = n .

1 B.A. unit = .98664 ohm.

$C = \frac{a}{ncd} \times \frac{1}{.98664 \times 10^9}$, C being the capacity of the condenser in absolute electromagnetic units.

$$\log c = \log 2,435,800 = 6.386642$$

$$\log d = \log 100,120 = 5.000521$$

$$\log n = \log 130.075 = 2.114194$$

$$\log .98664 \times 10^9 = 8.994159$$

$$22.495516$$

$$\log a = \log 1742.6 = 3.241198$$

$$\log C = 20.745682$$

$$\log C' = \log 50.069 = 1.699568$$

$$\log v^2 = 20.953886$$

$$\log v = 10.476943$$

$$\therefore v = 2.9988 \times 10^{10} \text{ cm. per sec.}$$

Table of Results.

[The numbers in the columns headed v , when multiplied by 10^{10} , give the values of v in centimetres per second.]

Group.	Date.	No.	v . Slow Fork.	Weight.	Weighted mean of group.	v . Fast Fork.	Weight.	Weighted mean of group.
Group I.	Mar. 28...	1	3·0040	2	3·0012 (24)	2·9994 2·9990 2·9975 2·9953 2·9977 2·9998 2·9986 2·9996	4 2 2 2 3 4 4 4	2·9987 (25)
	April 2 {	2	3·0031	3				
		3	2·9993	3				
		4	2·9980	3				
	" 4 {	5	3·0009	3				
		6	3·0010	2				
	" 5 {	7	3·0036	2				
		8						
	" 10...	9						
	" 12... {	10						
		11						
		12						
	" 13 {	13	2·9993	3				
		14	3·0031	3				
		15						
	" 16 {	16						
		17						
Group II.	May 4 {	18			3·0045 (16)	3·0048	3	3·0029 (33)
		19				3·0041	4	
		20				3·0053	4	
	" 6 {	21				3·0063	2	
		22				3·0029	4	
		23				3·0024	4	
	" 7 {	24				3·0006	4	
		25				3·0011	4	
		26				3·0009	4	
	" 8 {	27	3·0058	3				
		28	3·0007	3				
		29	3·0039	2				
	" 9 {	30	3·0069	2				
		31	3·0033	3				
		32	3·0073	3				
Group III.	May 13 {	33	3·0012	3	3·0043 (16)			
		34	3·0105	1				
		35	3·0090	3				
	" 14 {	36	3·0059	3				
		37	3·0021	2				
	" 15 {	38	3·0036	2				
		39	2·9990	2				
Group IV.	May 21...	40			3·0017 (15)	2·9947	1	2·9977 (25)
	" 22 {	41				2·9950	2	
		42				2·9966	2	
		43	2·9996	4				
	" 23 {	44	3·0025	4				
		45	3·0022	4				
		46	3·0028	3				
	" 24 {	47				2·9988	4	
		48				2·9978	4	
	June 6 {	49				2·9980	4	
		50				2·9980	4	
	" 8 {	51				2·9988	4	

The results exhibited in the preceding Table have been divided into four groups. The first group consists of seventeen values found before the condenser was taken apart to measure its electrostatic capacity. During this time the upper half of the shell was lifted, and the ball adjusted two or three times. The values found by the fast fork are more uniform than the others, and average somewhat lower. The second group extends from May 4 to 9 inclusive, when the condenser had been set up again. There were two small glass tubes, about 5 mm. in diameter (and drawn out considerably smaller where they projected through the shell one to two centimetres into the space within), which had once been used to pass charging-wires through. The wires had been withdrawn, and it was supposed that the glass tubes had no appreciable effect. The holes were together only $\frac{1}{5000}$ of the area of the shell, and the tendency of the glass to slightly increase the capacity would tend to counterbalance the decreasing effect of the holes. When the condenser was set up the second time, the tubes were intentionally left out and the values of Group II. were noticed to be larger than those of Group I. No cause could be discovered for this increase (which indicates a *less* electromagnetic capacity), but the tubes were replaced and Group III. taken. The mean of this group is as large as that of the preceding group. The tubes were now again withdrawn, and the holes covered with gold foil, making the inner surface of the shell continuous. Group IV. gave values averaging almost exactly the same as Group I. The circumstances were alike in other respects so far as is known, the usual variation in the circumstances of the observations, as already explained, occurring in all the groups. I do not think the presence or absence of the tubes could affect the capacity appreciably; they were altogether too small, probably not filling over a thirty-thousandth of the space between the ball and the shell. But that there was a difference in the actual capacity of the condenser when Groups I. and IV. were taken from its value when II. and III. were obtained seems almost certain. As yet I have not become satisfied as to the cause of this difference; but it seems probable that, in putting the condenser together, some obstruction lodged between the two halves of the shell and prevented them from coming completely together. Had they been separated a few hundredths of a millimetre only, the difference in question would be fully accounted for. The surfaces of contact are very accurately ground and polished, and loosening the screws does not cause them to separate, as proved by the capacity remaining constant. That the low

fork should give higher values for v , which means a *lower* value for the capacity, than the high fork, is rather unexpected and not fully understood. The low fork gave only a quarter the current given by the high one, and was less steady in its action in proportion to the current; consequently the single observations were less reliable, but this alone does not account for the nearly uniform difference.

In view of the uncertainty as to the cause of the variations it is difficult to determine how best to combine the results. The weight of each single value of v in the Table is determined by considering the number of observations from which it is calculated, the uniformity of the separate observations, the steadiness of the spot, &c. If we give to Groups II. and III. one half the weight of I. and IV., in proportion to the sum of the weights of the separate values we have as the mean for the fast fork **2·9994** and for the slow fork **3·0023**. Giving now double weight to the results of the fast fork, on account of their greater accuracy and uniformity, we have, as a mean of all,

$$v = 3\cdot0004 \times 10^{10} \text{ cm. per second.}$$

Again, if it be found that the cause suggested is the true cause of the excess of groups II. and III., then those groups should be thrown out, and we should have **2·9982** and **3·0014** as the means, which would give for the mean of all

$$v = 2\cdot9993 \times 10^{10} \text{ cm. per second.}$$

These values are based upon the value ·98664 for the British Association Unit.

It is proposed to resume this investigation next winter, when more perfect insulation can be obtained, and several improvements in the details of the apparatus will be made. The smaller ball of the condenser will then be used also, and the cause of the difference in the values given by the two forks will be studied. Although we cannot yet say whether v is greater or less than 300,000,000 metres per second, it seems certain that it is within a tenth per cent. of this number, and it is hoped in the continuation of this investigation to narrow considerably further the range of uncertainty.

For convenience of reference the following values of v and of the velocity of light as found by different observers are added, the values of v being corrected to the value ·98664 for the B.A. unit:—

<i>v</i> , ratio of the units.		Velocity of light.	
¹ 1856. Weber and Kohlrausch	$3 \cdot 107 \times 10^{10}$	1879. Michelson	$2 \cdot 9991 \times 10^{10}$
² 1869. W. Thomson and King.....	$2 \cdot 808 \times 10^{10}$	1882. Michelson	$2 \cdot 9985 \times 10^{10}$
³ 1868. Maxwell	$2 \cdot 842 \times 10^{10}$	1882. Newcomb	$\left\{ \begin{array}{l} 2 \cdot 9986 \times 10^{10} \\ 2 \cdot 9981 \times 10^{10} \end{array} \right.$
⁴ 1872. M'Kichan	$2 \cdot 896 \times 10^{10}$	1874. Cornu	$2 \cdot 9850 \times 10^{10}$
⁵ 1879. Ayrton and Perry	$2 \cdot 960 \times 10^{10}$	1878. Cornu	$3 \cdot 0040 \times 10^{10}$
⁶ 1880. Shida.....	$2 \cdot 955 \times 10^{10}$	1880-81. Young & Forbes	$3 \cdot 0138 \times 10^{10}$
⁷ 1883. J. J. Thomson ...	$2 \cdot 963 \times 10^{10}$		
⁸ 1884. Klemencic.....	$3 \cdot 019 \times 10^{10}$		
⁹ 1888. Himstedt	$3 \cdot 009 \times 10^{10}$		
1889. W. Thomson ...	$3 \cdot 004 \times 10^{10}$		

¹ Weber and Kohlrausch, *Electrodyn. Maasbestim.*, *Abh. der Königl. Sächs. Gesellschaft der Wissensch.* v. p. 219 (1856); and Pogg. *Ann.* 1856.

² King, Report of the Committee on Electrical Standards, 1869.

³ Maxwell, *Phil. Trans.* 1868, p. 643.

⁴ Dugald M'Kichan, *Phil. Trans.* 1879.

⁵ Ayrton and Perry, *Journ. Soc. Tel. Engineers*, 1879, p. 126.

⁶ Shida, *Phil. Mag.* [5] x. p. 431.

⁷ J. J. Thomson, *Phil. Trans.* 1883, p. 707.

⁸ Klemencic, *Wiener Berichte* [3] lxxxiii. p. 88.

⁹ Himstedt, *Wied. Ann.* no. 9 (1888).

Johns Hopkins University, Baltimore,
June 15, 1889.

XL. *Researches on the Electrical Resistance of Bismuth.*

By EDMOND VAN AÜBEL, *Doctor of Science* *.

A GREAT many memoirs having been published relating to the influence of temperature upon the electrical resistance of bismuth, I think it advisable to sum up in a few lines the actual state of the question, in order to show the bearing of my researches on the solution of the problem.

In the present memoir I have examined the electrical conductivity of bismuth between 0° and 100° only; I will therefore confine myself to stating the results arrived at by physicists between the same limits of temperature.

Matthiessen†, in conjunction with von Bose and other physicists, has found the conductivity of bismuth at 0° to be 1·245, that of silver being taken as 100, and has expressed the conductivity at t° as a function of the conductivity at 0° by the formula

$$\lambda_t = \lambda_0 (1 - 0 \cdot 0035216 t + 0 \cdot 000005728 t^2).$$

G. Wiedemann, Matthiessen, Holzmann, and Vogt have

* Communicated by the Physical Society: read June 22, 1889.

† The works of the physicists, whose names will be mentioned later on, are enumerated in the *Treatise on Electricity* by Prof. G. Wiedemann, vol. i. 1882, p. 503 and the following pages.

also studied the effect of the composition of the alloys Bi—Sn, Bi—Pb, upon their electrical conductivity.

According to Matthiessen and Vogt, the electrical conductivity of the alloys of bismuth is modified by the first heating and the first cooling. If λ_0 be the original value, and λ_{01} the conductivity after cooling, we have the following results:—

	λ_0	λ_{01}
Pb—Bi (2.27 vol. Pb to 100 vol. Bi)	8.101	7.633.
„ (18 „ „ „)	4.558	4.565.

These variations doubtless depend upon permanent changes in the molecular structure.

The conductivity of molten bismuth increases as the temperature is lowered; it diminishes rapidly when the metal solidifies, according to the researches of Matteucci and Matthiessen, and increases again as the solidified mass cools. If small traces of tin or lead be added to the molten bismuth, according to Matthiessen, the conductivity at first diminishes, as in the case of solid metals, and afterwards increases.

Fr. Weber has found for the specific electrical conductivity of bismuth (C.G.S.) 0.838×10^{-5} ; and L. Lorenz has given for the same quantity the values

$$0.929 \times 10^{-5} \text{ at } 0^\circ; \text{ and } 0.630 \times 10^{-5} \text{ at } 100^\circ.$$

M. Leduc* has observed that the electrical resistance of wires made of an alloy of bismuth and lead increases when the temperature is raised. He has also observed a difference between the electrical properties of wires and of thin plates of commercially pure bismuth, which he attributes to the method of preparation and to the very different rates of cooling.

More recently† the same physicist has found the following results with bismuth which had been run into a tube and then slowly cooled. When the bismuth is heated for the first time, say to 100° , it undergoes an annealing the effect of which is to diminish its initial resistance about 30 per cent. During this operation, between the temperatures 0° and 100° , the following formula is obtained, which must be received with caution:—

$$r_t = r_0 (1 + 0.00344t + 0.0000077t^2),$$

in which r_0 and r_t are the electrical resistances at the temperatures 0° and t° respectively. This formula gives the value $+0.00421$ for the mean coefficient of variation of the resistance between 0° and 100° .

* *Journal de Physique*, [2] iii. (1884) p. 362.

† Leduc, Thesis for doctor's degree presented in June 1888 to the Faculty of Sciences of Paris.

Afterwards the resistance varies, between the same limits, according to the formula:—

$$r_t = r_0(1 + 0.00375t + 0.00008t^2),$$

and the mean coefficient of variation of the resistance between 0° and 100° is then 0.00155.

For a thin plate of bismuth, Leduc has found that after annealing the resistance decreases, between 0° and 70° , according to the following formula:—

$$r_t = r_0(1 - 0.00158t + 0.00000437t^2).$$

And this gives the number -0.00127 for the coefficient of variation with the temperature between 0° and 70° . The resistance of the metal thus prepared would be much greater than that given by Matthiessen, perhaps more than double the value found by him, no doubt because of the difference of molecular structure of the specimens examined.

In 1884, Prof. Righi* published a very remarkable paper, and we will sum up those of his conclusions which bear directly upon our researches.

1. The resistance of commercial bismuth increases on heating between certain limits of temperature, and decreases between others (generally it decreases at temperatures near the ordinary temperature), and, by constructing a curve of resistance, with the temperatures as abscissæ, and the specific resistances as ordinates, we obtain in general a curve in the form of an M. There is a maximum at a low temperature, then a minimum, then a second maximum a little below fusing point, and finally a second minimum after the change of state.

2. The resistance of commercial bismuth varies not only with the temperature, but also with the manner in which the piece has been prepared, and with the temper of the metal.

3. Chemically pure bismuth behaves like other metals; it is not sensibly affected by tempering, and at 0° its resistance compared with that of mercury is 1.15.

4. The difference between pure and commercial bismuth is due to traces of tin, which give to the latter properties similar to those by which steel differs from iron.

5. On adding to bismuth tin in increasing amounts, the specific resistance becomes much greater up to a maximum, and then diminishes.

6. The presence of tin in increasing amounts modifies the curve of resistance, in the same manner as a temper more and more hard would do.

G. Wiedemann† thinks that the phenomena observed

* *Journal de Physique*, [2] iii. (1884) p. 355.

† *Elektricität*, Bd. iv., ii., p. 1228.

by Prof. Righi can be partly accounted for by the discontinuity of the bismuth wires.

G. P. Grimaldi* has studied the thermoelectric properties of bismuth, and has confirmed the analogies which Prof. Righi has found between pure bismuth and iron, the bismuth containing tin and steel.

A. von Ettingshausen and W. Nernst† have obtained results which are recorded in the following Table, where x is the electrical conductivity in absolute measure, and α the coefficient of variation of the electrical resistance with the temperature. The plates denoted by Bi were made of absolutely pure bismuth; the other plates denoted by LI to LIV were made of an alloy with tin.

	Bismuth.	Tin.	x (G.S.).	α .
	Parts by weight.	Parts by weight.		
Bi	100		4.80×10^{-6}	-0.0012
LI	99.05	0.95	2.46×10^{-6}	$+0.0016$
LII	98.54	1.46	2.71×10^{-6}	$+0.0018$
LIII. ...	93.86	6.14	3.46×10^{-6}	$+0.0024$
LIV. ...	86.9	13.1	5.62×10^{-6}	$+0.0025$

The electrical resistance increases then (between 0° and 30°) for every alloy, when the temperature is raised; it decreases on the contrary for pure bismuth under the same conditions. On increasing the quantity of tin, the coefficient of temperature α also increases. The conductivity decreases rapidly by the addition of small quantities of tin, and increases again afterwards.

C. L. Weber‡ has found that the resistance of the metal under consideration at first decreased up to about 100° , the coefficient of temperature being -0.0006 ; and then increased up to the melting-point. The position of the minimum, however, is displaced by repeated heatings and coolings between 80° and 120° .

The same physicist has also observed that the electrical resistance of alloys of bismuth and tin, containing from 10 to 80 per cent. of bismuth, increases between 0° and 120° , as the temperature is raised.

* *Beiblätter zu den Annalen der Physik* (1889), No. 1, p. 25.

† W. Nernst, *Annalen der Physik*, Neue Folge, Bd. xxxi. p. 783 (1887); A. von Ettingshausen and W. Nernst, *Annalen der Physik* (1888), Heft iii. p. 474.

‡ *Annalen der Physik*, 1888, xxxiv. p. 576.

In his *Leçons sur l'Électricité*, Prof. Exner* mentions that when bismuth is heated the resistance generally decreases, and does not return to its original value on cooling, but reaches a value which is higher as the cooling is more gradual.

Ph. Lenard and J. L. Howard† have studied bismuth wires obtained by means of a screw-press; these wires were rolled into a spiral immediately, while the metal was still hot. They found that in the case of pure bismuth the electrical resistance increased with the temperature between 0° and 36° by 0·0052 for every degree Centigrade.

Finally, in a preliminary communication, published last year‡, I took up again the study of the question. Although bought at the best manufactories in Germany, all the bismuth that I used was very impure.

I examined bismuth slowly cooled, hardened bismuth, and finally compressed bismuth. The molecular structure exerts a great influence upon the electrical properties of different kinds of impure bismuth. All the alloys of bismuth and tin which I studied gave an increase of electrical resistance when the temperature was raised, although the bismuth specimens which entered into the composition of these alloys produced the opposite effect. I also proved, in these preliminary experiments, the great influence of lead as an impurity in bismuth.

If my first results and those of the other physicists be compared with the conclusions arrived at in my present treatise, the wide differences which can be caused by impurities in the pieces of metal under examination will be very striking.

The modes of preparing and of suddenly cooling rods of bismuth have been fully described in a preliminary communication§.

Analysis of different Bismuths.

We have measured the electrical resistances of several different bismuths, which we will designate by

Latest Brommsdorff,
Classen I.,
Classen II.,
Classen III.,
Classen IV.,
and pure electrolyzed bismuth.

The first is the metal as pure as it is possible to obtain it

* *Vorlesungen über Elektrizität*, Wien (1888), p. 404.

† *Elektrotechn. Zeitschrift*, 1888, Bd. ix., July, Part xiv.

‡ *Bulletins de l'Académie royale de Belgique*, 1888, 3rd Series, xv. No. 1 (Preliminary communication).

§ *Phil. Mag.* vol. xxv. p. 191; *Proc. Phys. Soc. Lond.* vol. ix. p. 124.

commercially. It has been supplied to us by the well-known chemical works of Herr Brommsdorff at Erfurt, who has taken all possible care in its preparation. We have so named this product to distinguish it from Herr Brommsdorff's other bismuths, which we treated of in our "Preliminary Communication."

The qualitative analysis of this metal, made by Prof. Classen, showed that it contains several impurities, principally copper.

The four following bismuths are products as pure as can be obtained by chemical methods of precipitation; they were prepared by Prof. Classen, who used every precaution. The metals Classen I., II., and III. were obtained by reduction from bismuth oxychloride, Classen IV. by reduction from bismuth nitrate.

Bismuth nitrate is a pharmaceutical product which, it appears, can be obtained in a very pure state in commerce.

These different samples were subjected to a minute spectral analysis, the results of which we will point out later, but we may mention here that they all contained lead. Small quantities of lead were always carried down with the precipitated bismuth; this experiment repeated even 13 times never resulted in a pure product. It appears then that *bismuth cannot be obtained absolutely pure by precipitation.*

The last bismuth which we examined, and which we have called "pure electrolyzed bismuth," was prepared by electrolysis. During the electrolysis of a solution of bismuth containing traces of lead, pure bismuth was deposited at the negative pole; and lead, in the form of peroxide of lead, at the positive pole.

As to the lead or tin which were used in the alloys, they came from Herr Brommsdorff's chemical works, and were sold to me as pure. Besides, they were introduced into the alloys in such small quantities that there was no occasion to take account of any impurities they might have contained.

Purification of Commercial Bismuth.

First Method.

About 250 grams of the metal were dissolved in HNO_3 ; the solution was brought to the boiling-point in a porcelain dish into which concentrated HCl had been poured. In order to transform all the bismuth nitrate into the chloride, the operation was repeated until the presence of HNO_3 could no longer be detected. The residue was then dissolved in HCl , and alcohol added in successive small quantities. Most of the lead was precipitated in the form

of lead chloride, which was removed by filtration. The solution of bismuth was then distributed among 10 glass beakers holding about 4 litres each. The bismuth was precipitated as oxychloride on addition of water. The precipitate was decanted and washed until no trace of HCl could be found. Then it was again dissolved in HCl, the bismuth precipitated as oxychloride, and washed as before. And this was done twelve times. The precipitate finally obtained was carefully washed again, then dissolved in HCl. The bismuth in this solution, to which water had been added, was precipitated by the addition of ammonia and ammonium carbonate. After decantation the precipitate was washed with water, until all the ammonia had disappeared, and dissolved in HCl. This precipitation was repeated three times.

Finally, the precipitate was dissolved in HCl, and the chloride transformed into the oxychloride by addition of water. The resulting precipitate, entirely freed from acid, was dried, mixed with KCN, and reduced. The metallic bismuth was again purified by a second fusion with KCN.

The bismuths Classen I. and Classen III. were both prepared under the direction of Prof. Classen in the laboratory for analytical chemistry at the Polytechnical School at Aix-la-Chapelle.

Classen I. was prepared by Herr Nörrenberg; Classen III. was obtained by Herr Magdeburg by means of the "Bismuth Purissimum" from the Schucharell works at Görlitz.

Finally, we are indebted to the kindness of Prof. Classen himself for Classen II.

Second Method.

The product used in this second method was the "bismuth subnitrate purissimum" of Dr. Marquardt, at Bonn, which was employed by Marignac in his determination of the atomic weight of bismuth.

One kilogram of this product was dissolved in HCl, and the solution was divided among 22 glass beakers, having a capacity of 4 litres each, and these were filled with water. The precipitate of oxychloride was washed with water until all the HCl was removed. The solution in HCl and the precipitation by means of water were repeated three times. Then the precipitate was again dissolved in HCl, precipitated with ammonia and ammonium carbonate, and thoroughly washed with water. These operations were also repeated three times.

Finally, the precipitate was once more dissolved in HCl, the bismuth oxychloride obtained by the addition of water, and reduced by means of KCN and soda.

Third Method.

The metal, after having been purified by the preceding methods, was subjected to electrolysis to take away completely any trace of lead. Prof. Classen, who is at present engaged in determining the atomic weight of bismuth, intends to describe this electrolytic method in detail.

Researches with the Spectrum.

To produce the bismuth spectrum a large Ruhmkorff's coil with a Leyden jar intercalated was used, and the spark obtained between rods of the metal. The spectrometer employed was one of Meyerstein's with a Schroeder direct-vision prism composed of five separate prisms. The slit was made rather open so as to give a very bright spectrum.

With a smaller dispersion, produced by a fine Merz prism, the spectrum was still more luminous. It was interesting to compare the method of observation which we have just described with that which consists in sending an electric discharge between platinum electrodes, in a tube containing a solution of a salt of the metal. This method was also used, the rest of the apparatus remaining the same.

It can thus be proved experimentally, and this is of some importance to chemists, *that the first method is much more sure and exact than the other.*

On examining the bismuth "Latest Brommsdorff" in the spectroscope, sodium and copper were found to be present. The two characteristic lines D_1 D_2 of sodium remained, even when the surface of the bismuth rods had been well cleaned by long immersion in nitric acid. The metal contained only traces of lead.

The other bismuths contained no copper and no sodium. The *traces* of lead, which were found in the bismuths Classen I., II., III., IV., and in the "Latest Brommsdorff," were all very faint; indeed we were not able to establish them with certainty, except by the following process:—

After having substituted, for the original rods of bismuth, other rods of lead, so as to produce a good spectrum of this latter metal, the point of cross section of the micrometer wires of the spectroscope was placed upon the most visible line of the lead spectrum, and then the rods of lead were replaced by those of bismuth.

The bismuth Classen I. showed the very characteristic line of lead, of wave-length 5610.4.

The same line was found in Classen II. and Latest Brommsdorff; but it was much more feeble. Classen III. and Classen IV. also contained lead.

To sum up, all these bismuths contained lead in variable amounts, Classen II. and Latest Brommsdorff containing the least.

But in the case of the bismuth obtained by electrolysis, no impurity was discovered by spectrum analysis.

This very minute spectrum analysis of products, prepared with the greatest care by such a distinguished chemist as Prof. Classen, warrants us in saying that *pure bismuth cannot be obtained with certainty by precipitation. The electrolyzed metal can alone be considered as chemically pure.*

In the successive precipitations the traces of lead are drawn down mechanically.

In electrolyzing the solutions of bismuth, in which the spectrum analysis had revealed traces of lead, a very thin but perfectly visible coating of peroxide of lead was found at the positive pole.

Calculation of the Absolute Values of the Electrical Resistances.

The mode of measuring the electrical resistance has been described in the preliminary communication.

In order to determine the absolute value of the electrical resistances at 0° of the tempered rods, it was necessary to calculate the mean section of these rods from their weight, density, and length.

No sensible error is committed in taking 9.82 as the density of bismuth or of the alloys which we have studied, as the lead and tin contained in the latter were only present in very small quantities. Besides, a much greater cause of error is the exact determination of the length of the rods of bismuth between the two solderings.

In the case of the slowly cooled rods the difficulty is greater, because the bismuth adheres very closely to the glass tubes in which it is contained*, and because these tubes cannot be considered as cylindrical. In spite of every precaution it is impossible to break the glass, so that the whole of the metal may be got out clear without breaking the rod of bismuth. So I measured with a spherometer the diameters of the bismuth rod at the two extremities and in the middle; and I considered the rod as formed of two truncated cones joined together at the small end. It was then easy to calculate the electrical conductivity at 0° ; for example, either using the formula given by Siemens (*Annalen der Physik*, vol. cx. 1860, p. 3, or F. Kohlrausch, *Guide de Physique pratique*, édition française, p. 223), or calculating the section of a cylinder whose height is the length of the rod of bismuth, and whose volume is that of the two

* Righi, *Journal de Physique*, 2nd series, vol. iii. 1884, p. 132.

truncated cones joined together. The section of this cylinder can then be taken as the mean section of the rod.

All the absolute values that I give further on are thus only approximations, but they approach very near to the true values.

Results of the Electrical Measurements.

The following Tables contain, in column W, the electrical resistances as I have measured them, that is to say in Siemens units, and at different temperatures given by a Centigrade thermometer. I have given also, for each bismuth, the specific electrical resistance R_0 in C.G.S. units at 0° temperature; and the mean coefficient of variation of the electrical resistance between 0° and 100° , *i. e.* the quantity K of the equation $R_t = R_0(1 + Kt)$.

Bismuths.

(1) Rods which have been slowly cooled.

Bismuths.	Tempera- tures.	W. Electrical resistances in U.S.	K. Variation for 1° between 0° and 100°.	R ₀ .
Latest Bromms- dorff	0°	0.1390	+0.00325	10 ³ × 109.90
	19	0.1462		
	49.1	0.1590		
	99.6	0.1840		
Classen I.	0	0.2875	+0.00076	
	19.1	0.2882		
	49.2	0.2922		
	99.6	0.3095		
Classen II.....	0	0.1615	+0.00299	10 ³ × 124.69
	16.7	0.1680		
	49.6	0.1828		
	99.5	0.2096		
(2) Classen III. 2nd rod.	0	0.2447	+0.00161	
	20.5	0.2500		
	49.8	0.2595		
	99.8	0.2841		
(1) Classen III. 1st rod.	0	0.2407	+0.00132	10 ³ × 156.74
	20.5	0.2439		
	49.7	0.2509		
	99.8	0.2725		
	0	0.2036		
	20.3	0.2063		
Classen IV. 1st rod.	99.8	0.2302	+0.00126	10 ³ × 170.07
	It is allowed to cool slowly.			
	0	0.2043		
	19.9	0.2066		
	It is heated to 100°, and allowed to cool slowly.			
	0	0.2048		
	20	0.2069		
	52.5	0.2136		
	99.6	0.2305		
It is allowed to cool again slowly, and it is found that the values remain constant.				

Table (*continued*).

Bismuths.	Tempe- ratures.	W. Electrical resistances in U.S.	K. Variation for 1° between 0° and 100°.	R ₀ .
Classen IV. 2nd rod.	0	0.1790		
	20.2	0.1806		
	99.8	0.1998		
	It is allowed to cool slowly.			
	0	0.1794		
	20	0.1810		
	It is heated to 100°, and allowed to cool very slowly.			
	0	0.1798		
	20	0.1810	+0.00113	10 ³ × 168.35
	52	0.1861		
Pure electrolyzed Classen Bismuth, 1st rod.	99.6	0.2000		
	It is allowed to cool again slowly, and it is found that the values remain constant.			
	0	0.1031		
	99.61	0.1490	+0.00447	
	It is allowed to cool very slowly.			
	0	0.1032		
	99.65	0.1491		
	It is allowed to cool again very slowly.			
	0	0.1032	from 0° to 19°.5 : +0.00412	
	19.5	0.1115	from 0° to 55° : +0.00426	
Pure electrolyzed Classen Bismuth, 2nd rod*.	55	0.1274	from 0° to 99°.72 : +0.00447	
	99.72	0.1493		
	0	0.0912		
	99.6	0.1322		
	It is allowed to cool very slowly.			
	0	0.0913	from 0° to 22°.1 : +0.00411	
	22.1	0.0996	from 0° to 56° : +0.00426	
	56	0.1131	from 0° to 99°.7 : +0.00450	
	99.7	0.1323		10 ³ × 107.99

* We think it will be interesting to give, for the pure electrolyzed bismuth, the values of the influence of magnetism on the electrical resistance.

ΔW being the difference between the electrical resistance in the magnetic field and outside of it.

Temperatures.	100 $\frac{\Delta W}{W}$.	Intensity of magnetic field.
0		
99.7	2.9 0.415	about 1560 C.G.S. units.

(2) Tempered Rods.

Bismuths.	Tempera- tures.	W. Electrical resistances in U.S.	K. Variation for 1° between 0° and 100°.	R ₀ .
Latest Brommsdorff.	0	0.0965	+0.00199	10 ³ × 139.86
	17.6	0.0988		
	49.3	0.1040		
	99.7	0.1157		
Classen I. ...	0	0.1745	-0.000603	10 ³ × 246.91
	17.5	0.1729		
	49.1	0.1687		
	99.7	0.1640		
Classen II. 1st rod.	0	0.1275	+0.00106	10 ³ × 166.66
	16.7	0.1287		
	49.7	0.1319		
	99.5	0.1410		
Classen II. 2nd rod.	0	0.1292	+0.00128	10 ³ × 157.48
	16.9	0.1310		
	50.3	0.1353		
	99	0.1456		
Classen II. 3rd rod.	0	0.1205	+0.00116	10 ³ × 163.40
	18	0.1215		
	99.8	0.1342		
	It is allowed to cool slowly.			
	0	0.1203		
	99.7	0.1343		
Classen III. 1st rod.	A second observation showed that the resistances now remain constant.			10 ³ × 204.50
	0	0.1135		
	19.7	0.1127		
	99.8	0.1139		
	It is allowed to cool slowly.			
	0	0.1128		
	20	0.1121		
	48	0.1114		
	99.8	0.1138		
	A second set of observations of the electrical resistances from 0° to 100° showed that the values remained constant.			
Classen III. 2nd rod.	0	0.1376	+0.00005	10 ³ × 208.33
	20	0.1363		
	99.3	0.1372		
	It is allowed to cool slowly.			
	0	0.1364		
	20	0.1355		
	48	0.1346		
	99.8	0.1371		
A second set of observations showed that the values of the resistances remained constant.				

Table (continued).

Bismuths.	Tempe- ratures.	W. Electrical resistances in U.S.	K. Variation for 1° between 0° and 100°.	R ₀ .		
Classen IV. 1st rod.	16·6	0·1301	It is allowed to cool rather slowly.	$10^3 \times 207·47$		
	49·6	0·1285				
	99·7	0·1301				
	0	0·1307				
	16·6	0·1295				
	49·3	0·1281	-0·00004			
	99·7	0·1302				
	A second investigation showed that the values of the resistances remained constant.					
	Classen IV. 2nd rod.	16·5	0·1507		It is allowed to cool rather slowly.	$10^3 \times 212·94$
		49·6	0·1486			
99·7		0·1501				
0		0·1512				
16·5		0·1500				
49·5		0·1479	-0·00008			
99·7		0·1500				
A second investigation showed that the values of the resistances remained constant.						
Pure electrolyzed Classen Bismuth. 1st rod.		0	0·0965	+0·00434		
		99·6	0·1382			
Pure electrolyzed Classen Bismuth. 2nd rod*.	0	0·0952	It is allowed to cool very slowly.	$10^3 \times 108·69$		
	99·6	0·1370				
	0	0·0950				
	21·9	0·1033				
	56·1	0·1175				
	99·7	0·1372				
	from 0° to 21°·9: +0·00399					
	from 0° to 56°·1: +0·00422					
from 9° to 99°·7: +0·00445						

* We think it will be interesting to give the value of the magnetic action for this rod of hardened bismuth, as we have done elsewhere.

Temperatures.	$100 \frac{\Delta W}{W}$.	Intensity of magnetic field.
0	2·89	about 1560 C.G.S. units.
99·7	0·402	

(3) Compressed Rods.

Bismuths.	Tempera- tures.	W. Electrical resistances in U.S.	K. Variation for 1° between 0° and 100°.	R ₀ .
Classen II. 1st rod.	20·6	0·1300		
	It was observed that the electrical resistance was permanently changed each time the rod of bismuth was heated. Finally, after having been heated from 0° to 100°, and been allowed to cool from 100° to 0° several times, constant values were found. We cannot tell what is the cause of this considerable decrease in the resistance, but it is certainly not due to an alteration of the connexions.			
	0	0·1250		
	22	0·1215	—0·00049	10 ³ × 236·96
	99·4	0·1189		
Classen II. 2nd rod.	20·5	0·1353		
	Same observation as above.			
	0	0·1352		
	21·9	0·1302	—0·00083	10 ³ × 251·26
	99·3	0·1241		
Classen II. 3rd rod.	0	0·1019		
	18	0·0978		
	99·8	0·0913		
	It is allowed to cool slowly.			
	0	0·1024		
	99·7	0·0916	—0·00105	10 ³ × 268·10
A further investigation showed that the resistances then remained constant.				

Alloys with Tin.

Alloys.	Tempera- tures.	W. Electrical resistances in U.S.	K. Variation for 1° between 0° and 100°.	R ₀ .
1st. Rods slowly cooled.				
Latest Brommsdorff bismuth + Sn. 0·5 gr. Sn to 100 gr. Bi.	0°	0·2670		
	19	0·2710		10 ³ × 458·71
	49	0·2715		
	99·6	0·2623		
Bi Classen II. + Sn. 0·53 gr. Sn to 100 gr. Bi.	0	0·6790		
	16·7	0·6917		10 ³ × 416·66
	49·6	0·6890		
	99·5	0·6540		
2nd. Tempered Rods.				
Latest Brommsdorff bismuth + Sn. 0·5 gr. Sn to 100 gr. Bi.	0	0·1960		
	17·7	0·2020		10 ³ × 346·02
	49·3	0·2083		
	99·7	0·2095		
Bi Classen II. + Sn. 0·5 gr. Sn to 100 gr. Bi.	0	0·1795		
	17·5	0·1844		10 ³ × 354·61
	49·2	0·1905		
	99·7	0·1915		
Bi Classen II. + Sn. 0·53 gr. Sn to 100 gr. Bi.	0	0·2561		
	16·7	0·2637		10 ³ × 349·65
	49·9	0·2735		
	99·5	0·2780		

Alloys with Lead.

Alloys.	Tempera- tures.	W. Electrical resistances in U.S.	K. Variation for 1° between 0° and 100°.	R ₀ .
1st. Rods slowly cooled.				
Latest Brommsdorff	0	0.2125		
Bi + Pb.	19.1	0.2122		
0.5 gr. Pb to 100 gr. Bi.	69.0	0.2127		
	99.6	0.2200		
Bi Classen I. + Pb.	0	0.4037		
0.5 Pb to 100 gr. Bi.	19	0.3887		10 ³ × 362.32
	49.1	0.3690		
	99.6	0.3530		
2nd. Tempered Rods.				
Latest Brommsdorff	0	0.1420		
Bi + Pb.	17.6	0.1425		
0.5 gr. Pb to 100 gr. Bi.	49.2	0.1400		10 ³ × 245.70
	99.7	0.1365		
Bi Classen I. + Pb.	0	0.1475		
0.5 gr. Pb to 100 gr. Bi.	17.5	0.1485		10 ³ × 274.27
	49.1	0.1470		
	99.7	0.1414		

Conclusions.

I have observed that the electrical resistances of some rods were changed permanently after the first heating; but when once the values remain constant they do not change again, even after several months. Dr. Leduc* has observed a similar phenomenon. Rods of the same bismuths, either slowly cooled or hardened under the same conditions, give nearly the same values for the resistance at 0° and for the coefficient of variation with the temperature. The methods of tempering and of slowly cooling remain sensibly the same for the different specimens. We must not then look to this cause to explain the great variations which have been observed between one bismuth and another.

The molecular structure, which I have changed by tempering and by compression, makes a great difference in the electrical properties of impure bismuths. On the other hand, tempering appears to have no action whatever on pure bismuth; thus, the electrical resistance at 0° is:—

* Thesis for doctor's degree, p. 30.

for electrolyzed bismuth, slowly cooled . . . $10^3 \times 107.99$,
 and for the same metal, tempered . . . $10^3 \times 108.69$.

The coefficient of variation with the temperature and the influence of magnetism are very nearly the same for these two specimens. The coefficient of variation with the temperature besides is *positive*.

In the case of impure bismuths the process of tempering causes the coefficient K to decrease, and even to become negative. The action of compression seems to be still greater.

As to the absolute values of the electrical resistance at 0° , they increase under the action of tempering and compression.

If we compare the results furnished by the electrolyzed bismuth with those given by the other bismuths, we see that the effect of traces of lead is to produce a diminution in the value of K , and an increase in the value of R_0 . It can also be seen that, in the case of impure metals, a high value for R_0 generally corresponds to a low value for K .

With regard to alloys of impure bismuth with lead and tin, the results prove that these latter metals tend to increase R_0 , and can even in certain cases give a negative value for K . But the molecular action, and above all the lead, produce a much greater effect than the tin.

The electrolyzed bismuth presents a peculiarity which is not seen in the impure kinds. The coefficient K remains sensibly the same at different temperatures between 0° and 100° , and this may be considered as a proof of the purity of the metal.

In short, one may say that, of all the methods both physical and chemical, the determination of the electrical resistance is certainly the most exact for ascertaining if the bismuth be pure, and above all if it contain no trace of lead. While spectrum analysis and the process of electrolysis have with difficulty discovered the existence of lead in the bismuth Classen II. for example, the study of the electrical resistance leads to very different results.

When one considers all the difficulties I have met with in procuring the pure metal, and the number of electrical measurements which I have been obliged to make, so to speak, uselessly, it is plain that, before studying the physical properties of a metal, it would always be wise to submit the metal to a very careful spectrum analysis, to assure one's self of its purity.

The differences, which I pointed out at the beginning, between the results of other physicists are easily explained by the rarity of pure bismuth.

Unfortunately I had not enough of pure bismuth to be able to study compressed electrolyzed bismuth and its alloys with tin and lead.

Considering the great influence of very small traces of lead, it is allowable to suppose that the presence of this foreign metal modifies the molecular structure of the bismuth to a considerable extent; unfortunately a microscopic examination could hardly give any result here.

Finally, if the results which we have obtained in this treatise be compared with those which we published in our "Preliminary Communication," it will be seen how much the conclusions deduced from the experiments may vary according to the nature of the impurities contained in metal. Also, the question of the variation of the electrical conductivity of bismuth with the temperature presents a *difficulty of a chemical nature*, and we strongly recommend physicists to make known in their works the method of preparation, and if possible the results of the analysis of the different bismuths which they have examined.

By means of the two following Tables my results may be easily compared with those of other physicists. It appears by the results that the bismuths studied by Messrs. L. Lorenz and Righi were pure.

Bismuths.	Slowly cooled.		Tempered.		Compressed.	
	R_0 .	K *.	R_0 .	K.	R_0 .	K.
Brommsdorff...	$10^3 \times 109.90$	0.00325	$10^3 \times 139.86$	0.00199		
Classen I.	0.00076	$10^3 \times 246.91$	-0.000603		
Classen II. <i>a</i> ...	$10^3 \times 124.69$	0.00299	$10^3 \times 166.66$	+0.00106	$10^3 \times 236.96$	-0.00049
II. <i>b</i>	$10^3 \times 157.48$	+0.00128	$10^3 \times 251.26$	-0.00083
II. <i>c</i>	$10^3 \times 163.40$	+0.00116	$10^3 \times 268.10$	-0.00105
Classen III. <i>a</i> ...	$10^3 \times 156.74$	0.00132	$10^3 \times 204.50$	+0.00009		
III. <i>b</i>	0.00101	$10^3 \times 208.33$	+0.00005		
Classen IV. <i>a</i> ...	$10^3 \times 170.07$	0.00126	$10^3 \times 207.47$	-0.00004		
IV. <i>b</i> ...	$10^3 \times 168.35$	0.00113	$10^3 \times 212.94$	-0.00008		
Electrolyzed Bi.	$10^3 \times 107.99$	0.00429	$10^3 \times 108.69$	+0.00422		

* It is to be observed that all the coefficients of K in this column are *positive*.

Observations.	Experimenter.	R.	K.
	Lenz.	$10^3 \times 82.36$	
	Matthiessen.	$10^3 \times 131.50$	
	Matthiessen.	$10^3 \times 125.70$	+0.00418
	F. Weber.	$10^3 \times 119.33$	
	L. Lorenz.	$10^3 \times 107.64$	+0.00475
Bi cooled slowly.	Leduc.		+0.00455
Bismuth plate.	Leduc.		-0.00127
	Righi.	$10^3 \times 108.49$	
Bismuth plate.	Von Ettingsh. & Nernst.	$10^3 \times 208.33$	-0.0012
	C. L. Weber.		-0.0006
	Lenard & Howard.		+0.0052
Bi cooled slowly.	Edw. van Aubel.	$10^3 \times 107.99$	+0.00429
Bi hardened.	Edw. van Aubel.	$10^3 \times 108.69$	+0.00422

I have great pleasure in expressing my thanks to Profs. Wüllner and Classen for having so kindly furnished me with the valuable materials necessary for my researches.

Physical Laboratory of the Polytechnic School,
Aix-la-Chapelle.

XLI. Notices respecting New Books.

Force and Energy. A Theory of Dynamics. By GRANT ALLEN.
(Longmans, Green, and Co.: 1888. Pp. xiv + 161.)

THOSE familiar with Mr. Grant Allen's more popular writings, whether in Natural History or Fiction, were hardly prepared for his appearance as the Author of a new theory of Dynamics, a branch of Science supposed to be among those the bases of which are most firmly established, and verified by thousands of instances of the accordance with its predictions of phenomena such as the recent "Occultation of Jupiter by the Moon, the disappearance taking place at 7 h. 4 m. August 7th, afternoon, at 25° from the vertex..."

In a prefatory "Apology" Mr. Allen explains that the present book is the development of "a little twenty-page pamphlet, bearing the same title, printed privately at Oxford in 1875 for presentation to a few physical specialists." The result of the earlier appeal, we are told, was that "some said his theory was only what was already known and universally acknowledged; while others of them said it was diametrically opposed to what was already known"—a result not unlikely to happen, according as the specialist in dipping into the brochure chanced to alight on one or another passage.

The "summum genus" of Mr. Allen's theory of dynamical science is "POWER," of which "Force" and "Energy" are "sorts," the former "initiating or accelerating aggregative motion, while it resists or retards separative motion;" the latter "resisting or retarding aggregative, while it initiates or accelerates separative motion." "Gravitation, Cohesion, Capillarity, and Chemical Affinity are Forces; Heat, Electricity, and Light are Energies."

Phil. Mag. S. 5. Vol. 28. No. 173. Oct. 1889. 2 C

Here, then, "Force" is used in the ordinary sense of "Attractive Force," but "Energy" in a sense wholly different from its usual and accepted meaning, the power of doing *work*, the result and equivalent of the *work* done on the body in which the energy is said to reside. It may be remarked that Mr. Grant Allen's new theory ignores *work* and *momentum* wholly. Instances adduced in illustration of the above novel definition of Energy are, that "the Moon is prevented from falling upon the Earth and the Earth from falling into the Sun by the Energy of their respective orbital motions;" and that "a ball shot from a cannon into the air is prevented from falling by the Energy of its upward flight." Now if in these instances for "Energy" is substituted its new definition, "a Power accelerating separative motion from the Earth" (or Sun), the total irreconcilableness of the "theory" with what is well known is at once apparent. If Mr. Allen had contemplated the case of the gun being pointed from above downwards (say from the "fighting-top" of a ship's mast upon boarders on deck) he must have seen himself that the Energy of the shot would have been the same in amount as before, but its effect would have been "aggregative" in his sense; while the smaller Energy of the gun itself would have been "separative."

In these and such cases Mr. Allen fails altogether to realize the conditions of the problem; but in mere descriptions of phenomena (which occupy the greater part of the book) the facts are correctly stated with characteristic lucidity and charm of style*. If therein, with the alteration of a word or phrase occasionally, Energy were understood in its accepted sense all would be "just what was already known and universally acknowledged." Much that may have appeared to Mr. Allen as he wrote to plausibly support his notion of a "separative Power" (in a retrospective sentence at the opening of the concluding chapter is reiterated "our theory of two opposing Powers, aggregative and separative") is perhaps due to the selection of instances wherein disintegration is an attendant circumstance: thus (p. 118) the Energy of the prime mover in the water- or wind-mill is ultimately given up "partly in producing separation, in opposition to cohesion, among the molecules of corn." It does not seem to have occurred to Mr. Allen that it might, by a slight change of mechanism, have been employed in working a small hammer which should have produced the opposite effect among the "molecules" of a bar or rod of iron.

A tolerably good idea of what the book is as a whole might be derived from the supposition that Mr. Grant Allen had proposed to himself to rewrite the late Balfour Stewart's 'Conservation of

* Exception being made of occasional lapses into the "slipshod," such as the following found in chap. v. of part ii.:—"so soon as we apply heat to either, *they* burn away": "it is probable that they *spontaneously* decompose . . . on any direct contact with external agencies." An instance of lapse into incorrectness occurs in chap. viii.: (the Earth's) "orbital Energy and *nutation* which indirectly yield the phenomena of winter and summer;" where *the inclination of the plane of the orbit to that of the Equator* is evidently meant.

Energy,' adapting it to his peculiar "theory of two opposing Powers;" omitting all reference to *work* done by Force and introducing the principle of the "Persistence of Force," in the sense that "the total amount of Force or Aggregative Power in the universe is always a fixed quantity"—a notion which at one time so much exercised the mind of the great Faraday. Even "electrical units" when free are described as "rushing at once into a state of aggregation with their fellows"! A notice of this book should not conclude without mention of the perfect modesty—nay "timidity"—with which it is offered to the public. "If I am wrong," Mr. Allen assures us, "I shall expect to be frankly told so: I shall accept demonstrations of my mistakes and misconceptions with a good grace." But ingenuously, though illogically, he adds: "Naturally I shall continue still to think myself right." To *demonstrate*, however, to Mr. Allen's conviction the errors of his theory would involve his submitting himself to a strict, if not an extensive, course of training in orthodox dynamical science, which being mastered the scales would at once fall from his eyes, even if he should be forced to exclaim "Pol, me occidistis, amici, Non servastis . . . cui sic extorta voluptas, Et demptus per vim mentis gratissimus error!"—J. J. WALKER.

XLII. Intelligence and Miscellaneous Articles.

ON THE DEPENDENCE OF THE ELECTROMOTIVE POSITION OF
PALLADIUM ON THE QUANTITY OF HYDROGEN IT CONTAINS.
BY MAX THOMA.

THE difference of potential of palladium wires charged with hydrogen, in dilute sulphuric acid, against zinc was determined by comparing the deflexions of a Wiedemann's galvanometer of 12,000 ohms' resistance, as well as a Mascart's electrometer, with that of a Daniell's element $E = 1.07$ volt. The metals were placed in glasses, which were connected with a third one between them filled with dilute sulphuric acid.

The charge with hydrogen was defined by the expansion of a palladium wire. The wire, along with the wire which served as anode, was passed through a cork into a glass tube, and was bent below into a loop and held by a glass rod. A thin copper wire was attached to the palladium wire, which passed over a pulley and was loaded with a kilogramme. A mark showed the elongation.

The electromotive force of palladium towards zinc, so long as all hydrogen is absorbed by it, is independent of the absolute quantity of hydrogen (0.65). Palladium-hydrogenium is formed. If in this way the palladium has been entirely changed into the latter, on further charging with increase of hydrogen it continually approaches zinc, but still remains electronegative towards it. It reaches the power 0.39 D. After opening the polarizing current the polarization of the supersaturated palladium rapidly diminishes; and after a minute, even when the circuit was not closed, to the values of (0.65–0.67) of the supersaturation.—*Zeitschrift für physik Chem.* vol. iii. p. 69 (1889); *Beiblätter der Physik*, vol. xiii. p. 529.

THE ENERGY OF THERMAL RADIATION AT A WHITE HEAT.

BY O. TUMLIRZ AND A. KRUG.

The authors endeavoured to determine the energy which a platinum wire, heated by the current, gives off as light; and, in the first case, indirectly, by comparing the total quantity of heat imparted by the wire to a calorimeter with the energy imparted by the work of the current.

The difference found did not exceed the magnitude of the probable errors of the two determinations; by which on the one hand Joule's law was confirmed afresh, and on the other hand the loss of energy by radiation is infinitely small in comparison with the total loss of heat. The internal conduction in the wire plays an important part. In the course of this set of experiments it was found that the disintegration of the platinum heated by the current must have a disturbing influence over all such experiments; it makes the surface rough and thus increases the emissive power.

In order to measure directly the energy of radiation the authors used an air-thermometer specially constructed for the purpose, the theory of which is fully described and discussed.

The greatest radiation obtained represented an energy of 0.944 calories per square centimetre per second.

If we assume with Tyndall that the ratios of the energy of the visible to that of the whole spectrum is as 1 : 24, it follows that the energy of radiation corresponding to the light can heat 42 mg. in a second through 1° C.; and experiments made previously by the authors showed that, in the brightness of a platinum wire equivalent to the anyle-acetate unit of light, 0.00326 gramme-calorie were radiated in each second from a square centimetre.—*Wiener Berichte*, vol. xcvii. p. 1521; *Beiblätter der Physik*, vol. xiii. p. 499.

CALCULATION OF THE MECHANICAL EQUIVALENT OF LIGHT FROM THE EXPERIMENTS OF JULIUS THOMSEN. BY O. TUMLIRZ.

The author reverts to the experiments published in 1865 by J. Thomsen in Poggendorff's *Annalen*, vol. cxxv. p. 348, in which a bulb filled with warm water was placed at a certain distance from a thermopile; and thus various flames were placed at the same distance, the total energy of radiation of which was found from the indications of the galvanometer, the thermopile having been graduated in the first experiment on the basis of Dulong's law of radiation. By interposing a layer of water the rays of heat were cut off, and the proportion of the radiant energy due to light-rays was calculated. On the basis of the numbers given by J. Thomsen the author, after accurately defining the unit of light, calculates the radiation corresponding to the photometric unit of light at 0.0056 gm. cal., or the amount of work at 1 gr. \times 240 centim.

In this the flame of a sperm-candle which burns 8.2 grammes in one hour is taken as unit.—*Wiener Berichte*, vol. xcvii. p. 1625 (1888); *Beiblätter der Physik*, vol. xiii. p. 500 (1889).

THE
LONDON, EDINBURGH, AND DUBLIN
PHILOSOPHICAL MAGAZINE
AND
JOURNAL OF SCIENCE.

[FIFTH SERIES.]

NOVEMBER 1889.

XLIII. *On the Clark Cell as a Source of small Constant Currents.* By RICHARD THRELFALL, M.A., *Professor of Physics, University of Sydney*, and ARTHUR POLLOCK, B.Sc.*

[Plates XII. & XIII.]

THERE are several methods by which the potential-difference (for the future written P.D.) of the plates of a voltaic cell may be measured. To measure the E.M.F. it is, as a rule, necessary to prevent the cell under examination from yielding any appreciable current while the estimation of its terminal P.D. is being made.

The electrometer is consequently the most obvious instrument to use in such an investigation; but, as a matter of fact, it is infinitely inferior both in convenience and accuracy to the galvanometer.

All the experiments about to be described were made with a galvanometer in the manner adopted by Lord Rayleigh and Mrs. Sedgwick (Phil. Trans. ii. 1884, "On the Electrochemical Equivalent of Silver, and on the Absolute Electromotive Force of Clark Cells"). The method admits of an estimation of the terminal P.D. of a cell, either at rest or when (by the use of separate sets of resistance-coils to short-circuit its terminals) it is yielding a current of any desired value.

Let E be the E.M.F. of the cell, $P.D.$ its terminal potential-difference, C the current passing through it, R its internal resistance, R' the external resistance short-circuiting its terminal, Δ the observed change of P.D. at any instant when the

* Communicated by the Physical Society: read March 23, 1889.

current is flowing, measured from the value of P.D. when the cell is at rest. Then

$$\begin{aligned}\Delta &= \text{P.D. when cell is at rest} - \text{P.D. when cell is giving} \\ &\quad \text{current } C, \\ &= E - (E - CR), \\ &= CR \text{ when there is no polarization.}\end{aligned}$$

Now at any instant let the E.M.F. change by polarization so that it becomes E' , and let the internal resistance become R'' . Then, on short-circuiting,

$$\begin{aligned}\Delta &= E - (E' - C'R''), \\ &= E - \frac{E'R''}{R' + R''}.\end{aligned}$$

Consequently, if E' diminishes or R'' increases, we shall have increased values of Δ . These increased values will then indicate what is generally known as polarization. It is possible, however, that there may be another sort of polarization (if we may venture to so extend the term) of which an increasing E' or diminishing R'' is characteristic. If such an action exists and tends to increase as time goes on, we shall observe a continual decrease of Δ as time progresses after short-circuiting.

Such an effect has been pretty constantly observed; and as it was not due to any change in the compensator, we are obliged to attribute it to a small variation in the Clark cell itself in one or both of the directions mentioned.

On referring to Wiedemann (*Die Lehre von der Electricität*, edition 1883, vol. ii. p. 708) we found a statement, on the authority of Hockin and Taylor (*Journ. Soc. Teleg. Eng.* viii. p. 282, to which we have not access), to the effect that an increase of thickness of a polarizing layer of hydrogen on mercury causes the polarized surface to become more positive. The method employed, and the statement that finally bubbles of gas may be given off by sufficiently diminishing the area of the mercury surface, suggests that the polarizations used in the experiments were very strong. The electrolyte was dilute sulphuric acid.

Some experiments were made by us, using a similar but improved method and with zinc-sulphate solution as an electrolyte. The sort of effect we were in search of was an increase of potential on one mercury surface of about 0.0002 volt. Every precaution was taken in securing mercury and sulphate of zinc identical with those used in Clark cells, and in checking the behaviour of the electrodes. An effect larger than that we were in search of could generally be detected on

altering the extent of the mercury surfaces, but we finally convinced ourselves that the capillary assumptions made were of such a nature as to require revision. This would in itself have involved extensive research, and we did not feel that the end was likely to prove in any way commensurate with the labour of reaching it; consequently we are unable to say whether the potential of the mercury surface does or does not rise with increased density of hydrogen deposition. In any case the small deviation of the value of Δ observed from its value, as accounted for by the cell resistance, shows that very little (if any) hydrogen really reaches the mercury surface. On the other hand, it is easy to see several reasons why R'' might become smaller sufficiently to account for the effect observed. It may be worth noting that the possibility of collecting evidence as to the existence or nonexistence of polarization when the change of Δ with flux of time is very small becomes a question of indirect evidence.

We did not habitually estimate changes of Δ of less magnitude than 0.000045 volt, and consequently what follows refers only to changes of greater amount than this. By our method the obvious test for polarization would be a measurement of the resistance of the cell at the actual moment when its terminal P.D. was obtained. This is experimentally impossible (as we consider) with the accuracy necessary, to say nothing of the continual changes in the value of the current taken from the cell which are to be expected during the adjustment of the bridge. The tests which we apply can be rendered evident most easily by means of a diagram. Consider first the case of no polarization, *i. e.* no change of E or R . If we plot values of Δ as ordinates when C is nearly constant, and is assumed to be exactly so, and times reckoned from the epoch of short-circuiting as abscissæ, then the resulting curve will be three sides of a rectangle if it is prolonged till the moment of reinsulating the plates of the cell. This in fact corresponds simply to the case of $\Delta = CR$. Now let polarization set in. The curve giving the value of Δ will rise or fall, and the amount of rise or fall will be a sort of measure of the polarization.

Again, let currents of different values be taken from the cell and observe whether $\Delta = KC$, where K is a constant (it is of course assumed that R cannot be got with sufficient accuracy by other means, and the determining of it in this way would beg the question). We shall show that when certain conditions are complied with, this is nearly but not quite true. Consequently to the high degree of accuracy which has been aimed at in our experiments, it is never true to say that the

Clark cell remains unpolarized when changes of K are considered as evidences of polarization. In order to exhibit the changes actually occurring in K , the following Table has been prepared. To get the value of C , corresponding to any observed value of Δ , the experimental P.D. was taken and divided by the external resistance. The table shows what the cell variations actually are—expressed as if they were all to be traced to actual changes of resistance. It will be noticed that the values of K increase as the current diminishes, showing that what is ordinarily called polarization is less with greater currents than with small. It is fair, however, to state that the variations of P.D. in the latter case were nearer to our experimental limit of discrimination, and consequently are to be treated as having less weight than those referring to greater currents.

TABLE I.

Table giving the value of K from the equation $\Delta = KC$.

Date.	External resistance.	Δ .	Terminal P.D.	Current.	K .	
1888.	L. ohms.	volts.	volts.	amperes.		
July 26	143	0.04730	1.38776	0.009704	4.874	
26	143	0.04590	1.38911	0.009714	4.725	
26	143	0.04545	1.38956	0.009717	4.677	
26	143	0.04606	1.38913	0.009713	4.741	
26	494	0.01593	1.41917	0.002872	5.545	
26	987	0.00729	1.42781	0.001446	5.039	
	1423	0.005217	1.430663	0.001005	5.189	Mean of 13 obs. during July & Aug.
July 24	2862	0.00254	1.43323	0.0005007	5.072	Mean of 2 obs.
24	4293	0.00180	1.43383	0.0003339	5.384	
Aug. 21	10000	0.00076	1.43538	0.0001435	5.295	Mean of 2 obs.
Oct. 22	10000	0.00079	1.43448	0.0001434	5.507	Mean of 4 obs.
July 31	20000	0.00058	1.43489	0.00007174	8.084	
31	30000	0.00045	1.43507	0.00004783	9.407	
31	40000	0.00036	1.43521	0.00003588	10.03	
31	50000	0.00027	1.43530	0.00002870	9.406	
31	60000	0.00023	1.43539	0.00002392	9.614	
31	70000	0.00023	1.43544	0.00002050	11.21	
31	90000	0.00018	1.43554	0.00001595	11.28	
Nov. 1	10000	0.000630	1.42837	0.00014284	4.415	Temp. 21°·6 C.
2	10000	0.000765	1.42853	0.00014285	5.355	Mean of 2 obs.
1	10000	0.000630	1.42107	0.00014211	4.433	Temp. 21°·7 C.
1	10000	0.000630	1.42107	0.00014211	4.433	Temp. 34°·2 C.
1	10000	0.000630	1.42117	0.00014211	4.433	Temp. 33°·9 C.
1	10000	0.000630	1.42117	0.00014211	4.433	Temp. 33°·0 C.
1	10000	0.000720	1.42158	0.00014216	5.064	Temp. 32°·0 C.
1	1450	0.00293	1.41997	0.00097931	2.991	Temp. 30°·9 C.
1	1450	0.00257	1.41913	0.00097868	2.626	Temp. 30°·6 C.
2	1450	0.00315	1.42605	0.00098351	3.203	Temp. 21°·7 C.

It is, however, also true that polarization is never sufficient to cause an uncertainty of 0·5 per cent. in the value of a current of nominally '001 ampere yielded by the kind of cell about to be described.

The greatest observed uncertainty during three months of continued testing and varying of conditions was 0·6 per cent.; of this probably only half at most was to be traced to polarization.

On an average the uncertainty did not exceed 0·1 per cent.; so that we consider that in stating that the Clark cell may be used with security to give currents of a value not differing by more than 1 per cent. from their nominal value we shall leave a really unnecessarily large margin for uncertainty in the actual value of a properly constructed external resistance.

In May 1888 we began to investigate the behaviour of the Clark cell when giving small currents, with a view to applying it to the calibration at any time of a certain galvanometer which we had designed for one of the hospitals of Sydney. We were aware that the Clark cell had the reputation of being easily polarized, but this seemed to us probably to arise from the fact that the cells as usually made expose very small surfaces of zinc and mercury. We therefore constructed some cells of considerable size in order to find out whether our idea was or was not correct. The cells constructed and tested were the following:—

(1) A set of 40 test-tube cells of the ordinary pattern. The tube of each cell was three inches long and half an inch in internal diameter. The area of surface of mercury exposed was about 0·2 square inch or 1·3 square centimetre. The cells differed considerably from one another as regards resistance, the average resistance being about 40 l. ohms. The positive electrodes were bits of "pure" zinc rod as supplied by the dealers, $\frac{1}{4}$ inch in diameter, but of rather variable lengths. The areas of the zinc surfaces immersed differed considerably in the different cells. These cells were first set up in June 1886, and have been tested at intervals ever since. Their behaviour is quite satisfactory with the exception of that of No. 18, which was out by about '001 volt from the beginning.

(2) Thirteen cells, similar to the above, made up in 1888 for the purpose of this investigation, and charged with a sample of the same mercury as used by Lord Rayleigh, *i.e.* taken originally from the same bottle. Our object in preparing these cells was to connect our work directly with Lord Rayleigh's measurements.

Two days after being set up, the mean E.M.F. of these 13 cells exceeded the mean E.M.F. of the 40 cells by 0.00045 volt.

Considering that the paste had not been neutralized, the agreement is closer than might have been expected.

(3) A cell, generally called the "master cell," made up in a glass cylinder about 12 inches high and $1\frac{3}{4}$ inch in diameter. The zinc plate, which was just small enough to pass down the cylinder, was cut from a sheet of ordinary commercial zinc, and had been amalgamated before being placed in the cell. The mercury surface was about three square inches in area. This cell was generally used to oppose the others.

(4) Two cells set up in glass jars similar to the above, but shorter and of greater diameter. Each of these cells exposed a mercury surface of about 12 square inches, and a zinc surface of nearly equal amount. As it was expected that considerable solution of zinc would occur in these cells, the plates were wrapped up in parchment paper. One of the cells was called the "large cell," and the other the "hospital cell," as it was actually in tentative use in the hospital during the progress of the testing, and was occasionally brought over by hand for comparison.

The investigation of the peculiarities of these cells was directed to the elucidation of the following points:—

1. The discovery of the most probable value of the E.M.F. of the cell at rest with a view to finding its terminal P.D. when yielding a given current.

2. The effect of the extent of mercury and of zinc surface in determining the maximum current which can be taken from any cell with a given allowable amount of uncertainty.

3. Does the value of Δ change with lapse of time reckoned from the epoch of short-circuiting? and if so what is the best time for making an observation?

4. Does the value of Δ change appreciably from day to day, the external resistance through which the cells are short-circuited remaining constant, or generally has the immediate previous history of the cell anything to do with the value of Δ in short-circuiting?

5. Has change of temperature any considerable effect on the value of Δ , all the other conditions remaining constant?

6. The reliability of the large cell to give .001 ampere through a platinoid resistance assumed correct at 15° C.

Method of Experimenting.—As already stated, the method described by Lord Rayleigh (*loc. cit.*) has been used throughout. All the measurements were made in a room with a

southern aspect, and therefore not subject to any sudden change of temperature. Light was admitted through a sheet of paper forming a window in otherwise closed shutters. The temperature lay between 14° and 16° C. during the winter months, in which the experiments were made. At the close advantage was taken of a hot day to repeat the investigation at 23° C.

The galvanometer generally used had a resistance of about six thousand ohms, and a sensitiveness of about four scale-divisions for 10^{-11} amperes by direct test. This was sometimes supplemented by another galvanometer of nearly sixteen thousand ohms resistance, and whose sensitiveness could be brought up to several scale-divisions for 10^{-13} amperes.

The smallest change of P.D. usually noted was that compensated by altering the resistance in the compensator by 0.1 ohm corresponding to 0.000045 volt.

In addition to the apparatus requisite for finding the terminal P.D. of any cell at rest, resistances were required for the purpose of short-circuiting the terminals of the cell. For this purpose a very large box of German-silver resistance-coils was used. All the coils had been previously compared with standards by Carey Foster's method, and were in fact adjusted by this method in the laboratory. The corrections are in no case large enough to require taking into account. The insulation of the various portions of the apparatus was carefully attended to by the use of blocks of paraffin and was very necessary.

For the determination of the most probable value of the E.M.F. of any cell, comparisons of all the cells were made by the method mentioned, and the mean value was taken as identical in absolute measure with Lord Rayleigh's mean value, *i. e.* 1.43500 Rayleigh volt at 15° C. Many complete comparisons were made during three months, partly to find the mean and partly to check the action of each cell. The large cell and hospital cell were compared with the small ones, sometimes directly and sometimes through the intermediary of the master cell. The changes of temperature occurring were too small to affect the difference between the cells within our limits of observation. One hundred and sixty comparisons were used in computing the probable values of the E.M.F. of the large cell and of the hospital cell at rest.

Nothing could have been more satisfactory than the behaviour of all the cells, though they were badly used between whiles. Suffice it to say that the result was almost the same whatever cell was used to oppose the others; and throughout the months of June, July, and August the mean E.M.F. of

the large cell has the value of 1.43588 Rayleigh volt at 15° C., and the hospital cell the corresponding value of 1.434608 volt under the same conditions.

The second point referring to the effect of variable zinc and mercury surface on the observed values of Δ with varying currents was practically the first one to be investigated. Without going into the large amount of numerical information collected, we may state the following results:—

When the change of Δ during five minutes after short-circuiting is less than or equal to about $\frac{1}{10}$ per cent. of its value immediately after short-circuiting, the currents stand in about the order of the extent of mercury surface exposed. From a study of the small cells amongst themselves, it was found impossible to give any satisfactory explanation (of an experimental character) of the large difference of the maximum possible currents (*i. e.* those leading to a change of P.D. less than $\frac{1}{10}$ per cent.) that were observed.

The mercury surface was practically the same in all these cells, but the zinc surface varied. The currents did not stand at all in the order of the zinc surfaces. The most important factor appeared to be the thickness of the layer of mercurous sulphate, but it was not the only one. If the layer was less than about a quarter of an inch thick, the cells were bad. The result of it all was that the most important thing besides having plenty of mercurous sulphate is to have large surfaces of mercury and possibly of zinc. For this reason the large cells which it was intended to use in practice received the greatest share of attention, and we shall confine ourselves to describing the phenomena exhibited by them.

The results of some typical measurements are given in the following tables and curves, which will be found to contain information as to the effect of previous history as well. It may be stated that it was found possible to estimate the value of Δ within less than half a minute after short-circuiting.

From our point of view, *viz.* the applicability of the cell as a source of current of 0.001 ampere, it is immaterial whether there is polarization or not so long as it is not of a creeping character—nor then if the rate of creep is constant.

The answer to the third question is from the practical point of view absolutely vital and has received most attention.

Table II. gives the experience we have had. As a result the time finally chosen at which to make an observation is two minutes after short-circuiting. The information contained in the Table is also given graphically in Plate XII., where curves are drawn with values of Δ as ordinates and time as abscissæ.

In Plate XIII. corresponding curves are drawn when the cell is yielding a current of nearly 0.01 ampere. They are drawn from observations taken on July 26 with a resistance short-circuiting the terminals of the large cell of 143 l. ohms. (See Table I.)

TABLE II.

Variations of terminal P.D. with time when large Clark cell is short-circuited through 1423 l. ohms.

Δ is the terminal P.D. immediately before changing the resistance between the poles from ∞ to 1423 l.o. minus the terminal P.D. at the times given in the 2nd column.

Date.	Time.	Δ .	Resistance between Poles.
1888. July 24. (1.)	h m s 3 30 0 30 20 32 0 35 0 37 0 40 0 45 0 51 0	volts. 0.000000 +0.004095 +0.004815 steady at this. +0.000595 +0.000405 +0.000360 +0.000360	Changed from ∞ to 1423 l. 1423 [ohms. 1423 Changed to ∞ . ∞ ∞ ∞ ∞
July 24. (2.)	3 57 0 57 20 59 0 4 2 0 4 0 5 0 6 0 7 0 7 20 9 0 10 0 12 0 15 0 23 0	0.000000 +0.004635 +0.004500 +0.004500 +0.004500 +0.004500 +0.004500 +0.000135 0.000000 -0.000045 -0.000090 -0.000135 -0.000135	Changed from ∞ to 1423 l. o. 1423 1423 1423 1423 1423 1423 Changed to ∞ . ∞ ∞ ∞ ∞ ∞
July 26. (3.)	3 7 0 7 20 8 55 10 0 11 45 12 0 13 10 14 20 16 0	0.000000 +0.005805 +0.006030 +0.006120 +0.006120 +0.000405 +0.000315 +0.000315	Changed from ∞ to 1423 l. o. 1423 1423 1423 1423 Changed to ∞ . ∞ ∞ ∞

Table II. (*continued*).

Date.	Time.	Δ .	Resistance between Poles.
1888. Aug. 8.	h m s	volts,	
	3 17 0	0.000000	Changed from ∞ to 1423 l. o.
	18 0	+0.006588	1423
(4.)	20 20	+0.006499	1423
	22 20	+0.006454	1423
	25 45	+0.006409	1423
	26 0	Changed to ∞ .
	27 0	+0.000269	∞
Aug. 8.	3 30 0	0.000000	Changed from ∞ to 1423 l. o.
	31 0	+0.006230	1423
(5.)	34 45	+0.006185	1423
	35 0	Changed to ∞ .
	35 20	+0.000226	∞
Aug. 9.	12 57 0	0.000000	Changed from ∞ to 1423 l. o.
	57 40	+0.006819	1423
(6.)	58 40	+0.006685	1423
	1 1 0	+0.006640	1423
	2 0	Changed to ∞ .
	3 0	+0.000315	∞
Aug. 20.	5 17 0	0.000000	Changed from ∞ to 1423 l. o.
	17 30	+0.005227	1423
(7.)	18 30	+0.005182	1423
	20 20	+0.005114	1423
	22 2	+0.005047	1423
	22 10	Changed to ∞ .
	23 0	+0.000180	∞
	25 0	+0.000090	∞
Aug. 20.	5 25 0	0.000000	Changed from ∞ to 1423 l. o.
	25 30	+0.005024	1423
(8.)	26 0	+0.004979	1423
	34 45	+0.004822	1423
	35 0	Changed to ∞ .
	35 30	+0.000090	∞
Aug. 21.	4 5 0	0.000000	Changed from ∞ to 1423 l. o.
	6 0	+0.004906	1423
(9.)	7 20	+0.004821	1423
	10 0	+0.004726	1423
	10 0	Changed to ∞ .
	10 30	+0.000180	∞

Table II. (*continued*).

Date.	Time.	Δ .	Resistance between Poles.
1888. Aug. 21.	h m s	volts.	
	4 12 0	0.000000	Changed from ∞ to 1423 l. o.
	12 45	+0.004636	1423
(10.)	13 20	+0.004591	1423
	15 0	+0.004571	1423
	15 0	Changed to ∞ .
	16 0	+0.000045	∞
Aug. 21.	4 19 0	0.000000	Changed from ∞ to 1423 l. o.
	19 15	+0.004641	1423
(11.)	21 0	+0.004551	1423
	21 0	Changed to ∞ .
	22 0	+0.00009)	∞
Aug. 21.	4 25 0	0.000000	Changed from ∞ to 1423 l. o.
	25 40	+0.004546	1423
(12.)	26 10	+0.004505	1423
	28 0	+0.004505	1423
	28 0	Changed to ∞ .
	28 20	+0.000135	∞
Aug. 21.	4 30 0	0.000000	Changed from ∞ to 1423 l. o.
	31 0	+0.004551	1423
(13.)	36 0	+0.004551	1423
	36 0	Changed to ∞ .
	37 0	+0.000135	∞

The scale of the curves is very big in order to bring out differences of observed values of Δ ; it will be noticed, however, that Δ was in all cases extremely small.

The general effect is seen to be a decrease of Δ as time goes on from the epoch of short-circuiting. This has already been discussed. For comparison a large Daniell cell was examined at the same time, and the dotted curve in Plate XIII. referring to it will show its unexpected inferiority. The cell exposed about 80 square inches of copper, and was allowed to work long enough to obtain a clean electrolytic surface; zinc-sulphate solution was used in the inner cell. The resistance of the cell was very low, and the curve taken is for its behaviour when short-circuited through 143 l. o. This gives a current of about 0.0067 ampere; and a current-density of 0.000012 ampere per square centimetre of copper. In the large Clark cell the curve for a current of about 0.01 ampere (condemning the cell for so great a current) corresponds to a

current-density of 0.00012 per square centimetre of mercury surface. The more nearly corresponding curves, taking current-density as the criterion of correspondence, are to be found by using one of the Clark-cell curves for 0.001 ampere. In this case the latter is infinitely superior, and is even superior when giving 0.01 ampere with ten times the current-density. The trouble and difficulty involved in setting up a Daniell cell would quite independently render it useless for our purpose, so the comparison was on all accounts pushed no further.

It will be noticed that the greatest fall of terminal P.D. was observed on August 9th and amounted to 0.006685 volt, being 0.001468 greater than the mean. Now, on July 26th occurred the smallest observed value of the cell at rest, this was 1.435415 volt. Suppose that in practice these causes of error (here observed on different days) conspired, we should have taken a real P.D. of 1.428730 volt for 1.430663 volt. The error thus introduced would have been .002 volt nearly, or an error of about 0.14 per cent.

Similarly taking the greatest observed terminal P.D. at rest and the smallest fall, the error the other way if these causes conspire amounts to calling a real P.D. of 1.43186 only 1.430663, or about 0.08 per cent.

It remains to discuss the effect to be expected from changes of temperature. The temperature-coefficient of the Clark cell is of course known, but we have as yet given no evidence as to any possible temperature-change of Δ other than what is to be expected from the known change in the cell itself. To test this matter, on November 1st the large cell was placed in a very large vessel filled with water at a temperature of about 38° C., and left there for a considerable time. The bath was well wrapped up, and during 37 minutes the temperature fell to 34°·2 C. when tested by a standard thermometer. The fall of temperature being so slow allows us to hope that the temperature of the cell during the experiments did not differ to any considerable extent from that of the water. Moreover the E.M.F. of the cell had again become constant and had fallen through 0.00729 volt. By comparison with the 40 cells, on October 20th, it was found that the E.M.F. of the master cell was 1.43356 volt at 17° C. By a comparison of the large cell with the master cell at the time of the experiment and at a temperature of 21°·5 C., its E.M.F. was found to be 1.42900 volt; consequently at 34°·2 C. the E.M.F. was 1.42171 volt. The results are given in Table III.

TABLE III.

Fall of terminal P.D. of Large Cell when short-circuited through 10,000 l. ohms.

Date.	Temperature.	Δ .
1888. November 1	21.6 C.	volt. 0.000675
„ 1	21.6 C.	0.000585
„ 2	21.7 C.	0.000765
„ 1	34.2 C.	0.000630
„ 1	33.9 C.	0.000630
„ 1	33.0 C.	0.000630
„ 1	32.0 C.	0.000720

Fall of terminal P.D. of Large Cell when short-circuited through 1423 l. ohms.

Date.	Temperature.	Δ .
1888. November 1	30.9 C.	volt. 0.002925
„ 1	30.6 C.	0.002565
„ 2	21.7 C.	0.003150

The result to be noted is that the value of Δ may be said to have changed in the practical case from 0.005 to 0.003 volt between 15° and 30° C.—a very small variation considering the complexity of the changes which might have taken place; consequently we need not fear that any ordinary change of temperature will introduce any appreciable error except that arising from the ordinary temperature-coefficient of the cell.

This experiment gives incidentally the value of the temperature-coefficient for rapid heating over the range comprised between 21.7 and 30.9 C. Assuming Lord Rayleigh's temperature-coefficient for the range of temperature between 15° and 21.7 C., *i. e.* for temperatures which had been acquired so slowly as to allow the zinc-sulphate solution to reach its proper concentration with complete certainty, we have the following:—

Lord Rayleigh's formula is

$$E = 1.435 [1 - 0.00077 (t - 15^\circ)],$$

whence it follows that (calling the mean E.M.F. of the 40 cells at $15^\circ \text{C.} = 1.43500$ volt) at 17°C. , the temperature of comparison, the mean E.M.F. of the 40 cells is 1.43279 volt; and hence the E.M.F. of the master cell, as it was 0.00077 volt greater than the mean of the 40 cells, $= 1.43356$ volt. At $21^\circ.5 \text{C.}$ the E.M.F. of the master cell (using Lord Rayleigh's coefficient) $= 1.42859$ volt.

At the time of the experiments on the large cell the temperature of the laboratory was $21^\circ.5 \text{C.}$ and E.M.F. large cell — E.M.F. master cell $= +0.000405$ volt; therefore E.M.F. of large cell $= 1.428995$ volts at a temperature of $21^\circ.5 \text{C.}$ Thirty-seven minutes after having been put in the bath the E.M.F. of the large cell was steady at $34^\circ.2 \text{C.}$ and E.M.F. large cell — E.M.F. master cell $= -0.006885$ volt, the master cell being still at $21^\circ.5 \text{C.}$ Hence the fall of E.M.F. of the large cell for a rise of $12^\circ.7 \text{C.}$ was 0.00729 volt, and its actual E.M.F. was 1.4217 volt.

Hence taking our standard temperature at $21^\circ.5 \text{C.}$, and using Lord Rayleigh's formula with the constant appropriate to $21^\circ.5 \text{C.}$, we have

$$1.4217 = 1.42900 \{1 - K \times 12.7\},$$

whence $K = 0.000402$ between 21° and 34°C. say, and for rapid heating.

There is no reason why this value, deduced from a somewhat rapid change of temperature, should agree with the value obtained by Lord Rayleigh in cases where the temperature varied much more slowly and was considerably lower. We have evidence from the history of the 40 cells to the effect that at all events up to 24°C. the *differences* of E.M.F. are practically the same as they are at 15°C. In the case to which these results have been applied the external resistance through which the cell has to work is composed partly of a copper and partly of a platinoid resistance-coil. The copper coil in this case belongs to the galvanometer and the error thereby introduced, though well within the limits laid down for this particular application, is much greater than it would have been had the galvanometer-winding consisted of German-silver or platinoid wire. There would in this or similar cases have been no objection to the use of such a winding.

At 15°C. , then, to get the most accurate current of 0.001 ampere from the large cell we ought to use an external resistance of 1430.7 l. ohms, if we ignore the difference between

the Rayleigh volt and the Congress volt, as we may very well do.

It is now left for us to determine what error a change of temperature of 8° on either side of 15° C. is likely to cause. In this country, New South Wales, we have to work pretty often at temperatures as high as 23° C., and we imagine that no one would work below 7° C. if it could be avoided; consequently we take these as our limits.

We have already found that perturbations of the P.D. of the cell itself may introduce an error in that quantity to the extent say of 0.2 per cent. (the greatest we got by combining the most aberrant observations of different days was 0.14 per cent.).

On the strength of the experiment above 30° C. and the experience with the small cells from 14° to 23° C., we assume that no change in Δ will occur otherwise than that dependent on the ordinary coefficient. This is not strictly legitimate for lower temperatures, but it is difficult to obtain them much lower here.

We know that we shall have two causes at work tending to change our current value; these will be in the same direction.

The E.M.F. will tend to diminish with increased temperature, and the resistance of the wire will increase.

The cell-resistance change is of course included in variations of Δ , and these we have shown to be negligible for practical purposes when the external resistance is large.

By combining the effects of change of resistance and of E.M.F. (using Lord Rayleigh's coefficient) we get a temperature-coefficient for the current yielded.

The temperature change of resistance of platinoid wire is about 0.022 per cent. per degree, and the change of E.M.F. when the cell rises or falls 8 degrees from 15° C. is about 0.0088 volt. Consequently the error arising from this cause is about 0.6 per cent. Adding all the possible causes of error together we find that the uncertainty *may* arise to 0.96 per cent., say 1 per cent., in the value of the current. It need hardly be added that of these causes of error the two latter are easily allowed for where accuracy is required, and the former is very small. We do not think that in such a case an error of more than 0.1 per cent. ought to be expected.

For ordinary purposes for which the current may be used an accuracy to within 1 per cent. is probably all that will be required.

We have to thank Lord Rayleigh and Prof. Ayrton for some valuable suggestions as to the mode of statement of the above results.

XLIV. *On Lightning and Gunpowder Magazines.*

By CHARLES TOMLINSON, F.R.S., F.C.S., &c.*

SOME time about the year 1845 I visited the Tower of London in company with a relative who was fond of architecture, and wished to inspect the remarkable Norman Chapel of St. John in the White Tower. We were informed that the Chapel was closed, on account of a great bulk of the Public Records being stored in it. The authorities, however, allowed us to view it from an end gallery, which was partly choked up with dusty bundles of paper. We were informed that the vaults below the chapel were used as a gunpowder magazine. At this time the Duke of Wellington was High Constable of the Tower. When some one asked whether he was aware that some of the most valuable historical records of the kingdom were placed over the gunpowder magazine, the Duke replied, "I don't see what harm the Records can do to the gunpowder"! In other words, he was keeper of the gunpowder but not of the Records.

Nevertheless the danger implied by the question was by no means illusory. In 1767 the Church of St. Nazaire, at Brescia in Lombardy, was struck by lightning, and a quantity of gunpowder belonging to the Republic of Venice, stored in the vaults of the church and estimated at 207,600 lbs. in weight, exploded, when not only the church but a considerable portion of the town was destroyed, and about three thousand persons were killed. When the account of this disaster reached England our Government became alarmed as to the safety of our gunpowder magazines at Purfleet, which were quite unprotected against the attacks of lightning, and the Board of Ordnance sought the advice of the Royal Society as to the best means of protection. The Council appointed a committee, consisting of Dr. Franklin, the Hon. H. Cavendish, Messrs. Watson, Robertson, and Wilson, who inspected the magazines, and found them to consist of five buildings, side by side, each about 150 feet long and 52 feet wide, built of brick and arched under the roof, and standing on a chalk foundation. The Report, which was drawn up by Franklin, recommended an efficient system of pointed metallic conductors for each magazine, terminating at the lower extremity in a well of water to be dug at the end of each magazine. All the members of the committee signed the report except Wilson, who objected to pointed conductors on the ground that they drew the lightning to the building; and he recommended blunt or

* Communicated by the Author.

knobbed conductors as effectually answering the purpose of “conveying away the lightning *safely*, without that tendency to *increase* or *invite* it,” that belongs to points. Wilson published a long defence of his view of the matter, and was ably answered by Nairne and Henley. Franklin admitted that “points drew electricity at a greater distance in the gradual silent way; but knobs will draw at the greatest distance a stroke.”

Wilson found many supporters of his views, and the time of the Society was long occupied with discussions as to the respective merits of sharp and blunt conductors. Some even adopted the idea, which has survived even to our own time, that lightning-rods attracted the lightning to the building they were intended to protect, and thus occasioned loss instead of protection. This idea was started by the Abbé Nollet, who was for some time an electrical authority in France. In his *Mémoire sur les effets du Tonnerre* he expressed his opinion in the following terms:—“Toutes ces pointes de fer qu’on dresse en l’air sont plus propres à nous attirer le feu du tonnerre qu’à nous en préserver.”

The Board of Ordnance, however, resolved to adopt the Report of the Committee, supported as it was by such powerful names, and accordingly proceeded to erect pointed conductors on the powder-magazines. The Report is dated 21st August, 1772. Now it so happened that on the 12th May, 1777, the Board-house at Purfleet was struck by lightning at a point upwards of 40 feet from the conductor. The damage was but slight, consisting chiefly of a few stones fastened by iron cramps being thrown down. A similar accident had occurred on the 17th June, 1774, when the chimney of a house at Tenterden was struck by lightning although another chimney 50 feet distant was furnished with a conductor. Such cases as these were made the most of by Wilson in favour of his knobbed conductors, although the whole contest between the *sharps* and the *blunts* involved a sort of fallacy, since, as Snow Harris puts it, “any termination which can conveniently be given to a conductor, even if it were a ball a foot in diameter, would be in relation to, say, a thousand acres of cloud, virtually, a pointed conductor.” The mistake made by both parties in this controversy was in supposing that a lightning-conductor attracted the lightning: whereas it is as passive as the rain-pipes destined to carry off the rain. Should a building or a ship form part of the line of least resistance between a thunder-cloud and the earth or the sea, the building or the ship will be struck; but if they are furnished with properly arranged conductors, the lightning will pass

down without doing any damage, just as the rain passes down the water-pipe. By "properly arranged" is meant a conductor so fitted up that at whatever part of the building or ship the lightning may happen to strike, it shall be conducted safely to the earth or the sea. The French at one time had a theory that a lightning-rod protected a surrounding space equal to a circle the radius of which has twice the length of the lightning-rod. But in the presence of such cases as the above, and others that might be cited, the radius of protection theory has been abandoned by competent observers.

But to return to our narrative. The continued opposition of Wilson and his party embarrassed the Board of Ordnance, and its members again applied to the Royal Society for advice. After considerable debate, a second Committee was appointed, consisting of the President and Secretaries together with Messrs. Henley, Lane, and Nairne. They examined the powder-magazines, and again reported in favour of pointed conductors. Whereupon Wilson contrived a number of experiments in support of his views, and forwarded a detailed account of them to the Board of Ordnance, which referred it back to the Royal Society. Several meetings were occupied in the reading and discussion of Wilson's paper, and at length another Committee was appointed, consisting of the Hon. H. Cavendish, Henley, Lane, Lord Mahon, Nairne, Dr. Priestley, and Dr. Watson, with instructions to test Wilson's experiments and conclusions, and report the result to the Society. The report was altogether unfavourable to Wilson, who became wroth at this further testimony in favour of Franklin's pointed conductors, and he endeavoured to persuade the Government that the Society as a body did not agree with the reports of its several committees. Whereupon the Board of Ordnance applied to the President to ascertain whether this were so. The reply was to the effect that the voice of the Society was usually expressed by its Committees, but that in the present case "the Society had no reason to be dissatisfied with the Report of its Committee."

England was at this time at war with her American Colonies, and as the American rebel Franklin was the inventor of pointed conductors, Wilson and his friends seized the opportunity of making the quarrel a political one. It is scarcely credible at the present day that at the time here referred to, those who advocated sharp conductors were to be regarded as rebels and republicans, while those who advocated blunt conductors were naturally good and loyal subjects. Franklin was at this time in France, supporting the claims of the colonists in that country. In a letter to Dr. Ingenhousz, dated 14th October, 1777, after referring to the report which he drew up in 1772,

and which expressed his own view and that of the other members of the Committee as to the efficacy of pointed conductors, he adds :—

“I have no private interest in the reception of my inventions by the world, having never made, nor proposed to make, the least profit by any of them. The King’s changing his *pointed* conductors for *blunt* ones is therefore a matter of small importance to me. If I had a wish about it, it would be that he rejected them altogether as ineffectual. For it is only since he thought himself and family safe from the thunder of heaven, that he dared to use his own thunder in destroying his innocent subjects.”

Meanwhile Wilson was supported by all so-called loyal persons, who took part in the King’s resentment against his revolted subjects. The King, as hinted in Franklin’s letter, is said to have ordered blunt conductors to be affixed to his palace, and even endeavoured to make the Royal Society rescind its resolutions in favour of pointed conductors. His Majesty is also said to have endeavoured personally to persuade Sir John Pringle to use his influence in support of Wilson, but that the President had properly replied that he could not reverse the laws and operations of Nature. To this the King said, “Perhaps, Sir John, you had better resign.”

It has often been asserted that the suggestion thus put into the King’s mouth was never uttered. Dr. Kippis, F.R.S., Pringle’s friend and biographer, has been frequently cited in proof of the denial. In reviewing a book of mine in the ‘*Athenæum*,’ the late Prof. De Morgan adopted this view, and we exchanged friendly letters on the subject. The fact is that Dr. Kippis uses very cautious language. He published in 1783 six discourses by Pringle delivered at the Royal Society, prefaced by a biographical sketch. After alluding to the annoyance felt by his friend at the quarrel over the lightning-conductors, he adds (p. lvii):—“Of this matter the present writer of his life can assert nothing from personal knowledge ; for though he was then in the habit of a strict intimacy with Sir John Pringle, he never heard from him any suggestion of the kind that has been mentioned.”

It must be borne in mind that Pringle was “Physician to their Majesties,” and that Dr. Kippis was a leading Unitarian divine, whose pen was largely employed on the ‘*Annual Register*,’ the *Biographia Britannica*, and similar publications, and that the position of Dissenters, and especially of Unitarians at the time, was by no means safe, as we learn from the writings of Locke at an earlier, and from the treatment of Franklin and Priestley at a later period. But what

we know of the King's conduct in this matter is so much in harmony with his character, that the truth of the anecdote is at least probable, seeing that the President apparently acted on the suggestion of the King, and resigned at the next anniversary meeting of the Society, while the Fellows passed a strong resolution in his favour. From a scientific point of view he was certainly better qualified for the office of President than his courtly successor, Sir Joseph Banks*. Pringle died in 1782, and Condorcet delivered his *Eloge*, which was based on the Memoir by Dr. Kippis.

I have ventured to revive this interesting episode in the scientific history of the lightning-conductor from the fact that even now it is a matter of discussion as to the best method of protecting various structures, especially powder-magazines, from the effects of lightning. The scientific journals and the newspapers frequently contain accounts of what are called the "vagaries," and "eccentricities," and "caprices" of lightning, just as if the whole subject consisted of a series of unsolved problems. Writers describe what they have seen and then ask for explanations. Trees are struck and shattered, and the bark and splinters scattered in all directions, and yet the reporter in each case fancies that he sees something unusual. A recent writer in 'Nature,' in endeavouring to account for such effects, has magnified the nitric-acid formation of the Cavendish experiment into the production of nitro-glycerine. In other words, if a flash of lightning passing through the air produces the synthetic result N_2O_5 , is it not also capable of producing the compound known by the complex formula $C_3H_5(NO_3)_3$?

* Cuvier, in his *Eloge* on Banks, says:—"The works which this man leaves behind him occupy a few pages only: their importance is not greatly superior to their extent; and yet his name will shine out with lustre in the history of the Sciences." That is, during upwards of forty years that he presided over the Royal Society, he promoted science by his wealth, rank, and influence. Cuvier's *Eloge* was translated by me in a work entitled 'Sir Joseph Banks and the Royal Society,' published by Parker in 1844. Banks long exercised considerable influence over the election of Fellows. He was accustomed to regard two sets of men as eligible for the Fellowship. In the first rank he placed working men of Science; in the second men of rank and wealth, who were likely to patronise Science. When Dr. Vaughan, a fashionable physician, wished to enter the Society, the President opposed him on the ground that he was not a working man of Science. But some years later when he inherited a fortune and became Sir Henry Halford, Banks consented to his admission as a patron of Science. Many of the older Fellows adopted the same view; and I heard it asserted on the occasion when the choice of President rested between the Duke of Sussex and Sir John Herschel, that the former should be preferred on account of his influence with the Government in furthering the interests of the Society.

The following case, which has been reported to me from Dresden, and has not, so far as I know, been noticed in any English journal, has produced the usual bewilderment. On the 15th May last the valley of the Upper Elbe was assailed by a violent storm which about midnight burst upon the town and fortress of Königstein. Flash followed flash, stroke followed stroke, the thunder was incessant, and rain fell in torrents. Suddenly towards one o'clock A.M. a thunder-clap louder than any before was heard, and it was found that the powder-magazine A had been struck by lightning and its contents exploded. The magazine contained, in addition to stores of gunpowder, upwards of a thousand grenades, shrapnel shells, and also cartridges. The roof of the magazine was carried off, and the rest of the structure blown up into the air, while beams and stones were hurled about far and near, together with fragments of exploding shells. A sentry-box, with a man in it, was whirled away: the man was injured in the head and arm, and lamed. The watchman also and another man were hurt. It is stated that a trifurcated stroke fell; one prong on the tower of the fortress, a second on the Law Court and Casino, and the third on the powder-magazine. The shells continued to explode during many hours, so that passengers had to be warned off. Among the neighbouring buildings a new canteen was destroyed, the roof of the stables was knocked into holes, the windows of the dwelling-houses completely smashed, both in the fortress and in the town. The open spaces within the fortress were covered with wreck, and fragments of cartridges were hanging on the trees all around. Indeed the effects of the explosion could be traced for miles. Persons at a distance, feeling their houses shaken, doors slammed, domestic utensils rattled, animals uneasy, and dogs barking, supposed that an earthquake had taken place.

The question now naturally arises, whether this powder-magazine was protected by means of properly arranged conductors. The military authorities state that the magazine had three lightning-conductors, which had been examined only a few days before the accident, and were reported to be in good order. The theory is that the roof together with the conductors was first carried off, and that the lightning then fired the powder. But in the account furnished to me, it is stated as a fortunate circumstance that there was very little wind at the time. If so, how could the lightning carry off the roof, together with the conductors? It is more reasonable to suppose that the conductors terminated in dry ground and so offered resistance to the lightning, so as to allow it to develop its tremendous heating and explosive properties. No inspection of the ruins

after such an explosion would throw much, if any, light upon the cause of the accident. As to the inspection of the conductors shortly before the explosion, it is necessary to see the report, if any were made, and also to know whether the inspectors were competent to undertake the task.

In the case of the Purfleet magazines, Franklin's arrangement of the protecting conductors was judicious, but the magazines themselves were faulty in construction. The five magazines were placed side by side with interspaces of about 57 feet. If an accident had happened to any one, the others would probably have shared its fate. The barrels of gunpowder were piled on each other up to the spring of the arches, and each barrel was bound with four copper hoops, while a number of iron bars passed through the arches to support the timbers on which a crane was worked, thus forming broken conductors within the building. These iron bars were, however, removed at the suggestion of some Fellows of the Royal Society, with the approval of the Committee.

Snow Harris admitted the difficulty of providing complete protection to powder-magazines, from the fact that they are frequently constructed in a series of long straggling buildings, and hence are more likely to be struck at some point distant from the conductors. He recommended as the safest form for a powder-magazine a circular building with a conical metallic roof, furnished with a projecting pointed rod from the vertex, the cylindrical body being furnished with several vertical metal rods, attached to the roof and passing into damp ground or water. If such buildings were covered on the outside entirely with metal, still greater security would be attained.

The late Professor Clerk-Maxwell was so kind as to communicate to me his ideas on the same subject. Under his system the projecting point and the ground connexion were dispensed with, the function of the point being rather to tap the thunder-cloud, or the atmospheric charge, than protect the building; whereas the object to be attained should rather be to prevent the possibility of a discharge taking place within a certain space, such as a gunpowder manufactory or magazine.

His system is based upon the postulate that an electric discharge cannot take place between two bodies unless the difference of their potentials be sufficiently great, compared with the distance between them. If we can keep the potentials of all bodies within a certain space equal, or nearly so, no discharge will take place between them. We may secure this by connecting all these bodies by means of good metallic con-

ductors. But it is not necessary to do so; for it may be shown experimentally that if every part of the surface surrounding a certain space is at the same potential, every point within that space must be at the same potential, provided no charged body be placed within that space. It would therefore be sufficient to surround a powder-magazine with a good conductor, to sheath its roof, walls, and ground-floor with thick sheet copper, and under such conditions no electrical effect could occur within it on account of any thunderstorm outside. In such case there would be no need of any earth-connexion; a layer of asphalt might be placed between the floor and the ground so as to insulate the building. Should the magazine be struck by lightning it would remain charged for some time; and a person standing on the ground outside and touching the wall might receive a shock, but no electrical effect would be perceived inside even on the most delicate electrometer*.

For ordinary buildings it is sufficient to enclose the building with a network, say, of copper wire, such as No. 4 British wire-gauge (0.238 inch diameter), carried round the foundation of the house, up each of the corners and gables, and along the ridges. The copper wire may be built into the wall to prevent theft; and it should be connected to any outside metal, such as the lead or zinc on the roof, and to metal rain-water pipes, &c.

Highgate, N.,
August 1889.

* On this subject see Faraday, 'Experimental Researches,' §§ 1173-4. An insulated cubical chamber was constructed, coated on the outside with metal, and charged with electricity from a powerful machine. "I put a delicate gold-leaf electrometer within the cube, and then charged the whole by an *outside* communication very strongly for some time together; but neither during the charge nor after the discharge did the electrometer or the air within show the least signs of electricity."

Going further back, we may refer to Priestley's 'History of Electricity' (4to, 1775) for some experiments by Franklin and the author. A pair of pith-balls suspended within an insulated tin quart vessel were not in the least affected when a charge was given to the vessel (pp. 688 *et seq.*). It was also found that a wire net could be substituted for the tin quart. Hence a wire netting would protect a gunpowder magazine as effectually as plates of metal, but would not be so durable.

XLV. *On the Mechanics of Luminosity.*

By E. WIEDEMANN.

[Concluded from p. 267.]

General Considerations as to the Coefficients of True and Total Emission.

33. **I**N connexion with these investigations, before passing to the calculation of the store of luminous energy, I wish to consider certain cases, in which the difference between the total and true coefficients of emission, or the brightnesses corresponding to them in the visible spectrum, must be taken into account. The difference between true and total brightness is clearly seen in a comparison of the band-spectra, such as are produced by the haloid compounds of the metals of the alkaline earths, and the line-spectra as shown by the alkaline metals themselves. The total emission in the band- and line-spectra is, as mentioned above, nearly the same; but the true emission of the line-spectra is much greater than that of the band-spectra, for with a dispersion which spreads out the bands to continuous relatively-feeble broad bands, the lines are still sharp and bright. If in the same spectrum there are bright lines and less bright lines, it does not follow that the latter correspond throughout to a smaller true emission if the brighter ones are not also the narrower. These two circumstances must be taken into account, for example, in the reversal of lines, since the absorption corresponds to the true emission.

A comparison of the brightness of lines in the spectrum, for which the dispersion does not extend so far as to expand them, can teach us little as to the true coefficient of emission, and the kinetic processes corresponding therewith. At most thus much, that at one point of the spectrum there exists a greater energy of motion than at another. This holds good, for example, for the investigations of M. Lagarde* on the total brightness of the hydrogen-lines. The increase in brightness in the separate lines produced by stronger discharges may be explained just as well by an expansion of the lines—that is, by the addition of new vibrations to the original ones—as by an increase in the true brightness, that is, by an increase in the energy of the original vibrations.

Determination of the Store of Luminous Energy.

34. We will now endeavour to determine the store of luminous energy L , upon which the luminous motions depend,

* *Ann. de Chim. et de Phys.* [6] iv. p. 248 (1835).

from the values found for the energies in absolute measure. According to p. 161, $L = E_0/b$, where E_0 denotes the energy emitted in unit time (i_0 is there used instead of E_0), whilst the condition of the body remains the same as at the beginning. E_0 has been already determined by the measurements already detailed. We must further know the value of b , which determines the velocity with which a body exposed to a cause producing light comes to rest again.

We evidently obtain the value b if we know the intensities J_t and J_0 , to which the radiated energies are proportional at a time 0 and a time t , according to the equation

$$J_t = J_0 e^{-bt} \quad \text{or} \quad b = \log (J_0/J_t)/(t \log e).$$

For this calculation, of course, those phenomena are not to be employed in which, although a gradually diminished emission of light occurs, the decrease of brightness does not depend only on the diminution of the luminous motions, but the radiated luminous energy is partly replaced by other processes.

This takes place in a body glowing in consequence of an increase of temperature, which gradually cools by radiation. For although, by cooling, the molecules do at first lose a portion of the energy of these vibratory motions, if the energy of the luminous motion falls below the amount corresponding to the temperature, a portion of the same is restored by the impacts of the molecules at the expense of the motions of translation.

If, further, the body absorbs much light, and if the layer used is so thick that all of its molecules cannot emit rays, then a portion of the energy of the inner, hotter, not radiating layers will be conducted to the outer cooler layers, which are cooling by radiation. In these cases, therefore, not only the energy contained in the *form of luminous motions* is lost, but the total quantity of heat which was communicated to the body by heating from the surrounding temperature to another higher temperature. Measurements of the times of cooling which, for example, gave 1.2 second for a glow-lamp to cool from a white heat to darkness, and about 8 seconds for a platinum wire, 0.3 millim. thick, cannot be employed at once for the determination of our magnitude b .

Investigation of Balmain's Luminous Paint.

35. Processes in which we can assume radiation in consequence of the luminous motions present, without such secondary conditions, are phenomena of phosphorescence. I have therefore investigated, in the first place, how far this assumption in the case of the ordinary phosphorescent sub-

stances, *e. g.* the calcium-sulphide combinations, agrees with the experimental results as to the diminution of brightness. At the same time it was determined how much of the absorbed energy was again radiated as light.

Balmain's luminous paint was employed as the phosphorescent substance; it was formed into a plate by mixing the powder with aqueous gelatine, and drying the mixture on glass plates provided with a paper border. In this way very fine homogeneous thin films were obtained, which phosphoresced well.

The following experimental arrangement was made. The rays coming from a Schuckert's arc lamp, *L* (Plate III. fig. 4), fell upon the slit, *S*, of a spectroscope; the collimator-lens, *C*, rendered them parallel; then they traversed a prism, *P*, and an achromatic lens, *A*, of 1 metre focal length, which threw a real spectrum upon a second slit, *S*₁ (the observing-telescope was turned to one side). From this spectrum the slit *S*₁ admitted the strongly active portion of the ultra-violet rays, which fell then upon a plate, *B*, of Balmain's paint supported on a glass plate, *G*, at an angle of 45° to the direction of the rays. From the phosphorescent light emitted in all directions a cone of rays was allowed to pass through a small opening, ω , in a screen, *Q*, which was placed parallel to the rays passing through *S*₁, of which the rays were rendered parallel by the lens, *L*₁. The objective, *O*, of a Zöllner's astro-photometer produced a sharp image of the small screen, if this were illuminated. If the luminous paint had been rendered phosphorescent, then there was seen in the field of view of the photometer a sharp image of the screen, together with the comparison-star. This was made of similar colour to the first by means of a colorimeter, and was then reduced to equal brightness.

After the rest of the apparatus had been adjusted, the plate of Balmain's luminous paint, which had been a long time in the dark, and which showed no trace of luminosity even in a perfectly dark room, was put into its place by the aid of a small reading-off lamp, which caused no perceptible excitement. The light of the electric lamp was completely shut off by means of a screen placed immediately in front of the collecting-lens. One observer then removed the screen from the lamp during a definite time, as determined by the beat of a seconds pendulum, and thus allowed a definite quantity of light to fall upon the luminous tablet, whilst another observer sat in front of the photometer with closed eyes. As soon as the screen had been replaced the second observer adjusted the photometer as quickly as possible, so that the comparison-star

and the luminous colour appeared equally bright. The time at which this adjustment was completed was read off on a chronoscope. Then adjustments were made at short intervals at the commencement of the experiments, when the brightness changed rapidly; later, when the changes in brightness occurred less rapidly, at longer intervals. In order to be able to determine the brightness of the phosphorescence as rapidly as possible in the first few seconds after screening off the exciting light, the proper position of the Nicol prism of the photometer was determined approximately by preliminary experiments, and in the final measurement only the last adjustments were quickly made. The intensities i are proportional to $\sin^2 \alpha$, in consequence of the choice of zero-point on the photometer (the comparison-stars are dark for $\alpha=0$); we put $i=10^3 \sin^2 \alpha$. After the tablet had been illuminated for one second the following angular measurements were obtained at the times t on the rotating graduated arc, carrying the Nicol prism of the Zöllner's photometer:—

Series I.

t	4".	20".	40".	1' 3".	1' 30".	3'.	4'.
α^0	9.6	5.0	3.7	3.2	2.5	1.8	1.8
i_1	27.8	7.6	4.2	3.1	1.9	1.0	1.0

Then after three minutes' illumination:—

Series II.

t	...	3".	15".	25".	40".	55".	1' 10".	1' 40".	2'.	3'.	4'.	5'.
α^0	...	24.8	17.1	12.2	8.2	7.3	6.4	5.4	4.7	4.1	3.6	3.6
i_2	...	17.5	86.5	44.7	20.5	16.1	12.4	8.9	6.7	5.1	3.9	3.9

These numbers, as well as numerous others, and a graphical representation of the intensity as a function of the time, show that the falling off from a definite brightness of the tablet is dependent upon the duration of the previous illumination. If this was only very short (*e. g.* 1 second), the falling off is much more rapid than when the illumination has lasted longer or has been very intense. In Series I., for instance, with short illumination, the brightness sinks from 27.8 to 7.6 in 16 seconds; whilst in Series II., with long illumination, the brightness sinks from 20.5 to 8.9 in 60 seconds, &c.

In a substance incapable of undergoing change that is heated to luminosity, *e. g.* a glowing platinum wire, the change in brightness, observed from any brightness J , must be independent of the maximum brightness, which, for example, the body had before it, by cooling, reached the brightness J . The phenomena with the luminous paint thus

do not correspond to a simple emission of the luminous motions previously present in the body. We cannot therefore employ experiments with Balmain's paint or similar substances for the determination of our quantity *b*.

If we endeavour to explain these phenomena we may make the following assumptions.

The incident light brings about chemical changes in the phosphorescent body, which afterwards undergoes the same changes in the reverse order, and thus gives rise to an evolution of light. The chemically altered substance immediately on the surface would transmit those rays which were absorbed by the original modification, and thus deeper layers of the substance would become accessible to the action; exactly as, for example, in mercuric iodide the yellow modification is transparent to other rays than the red modification, and *vice versa*. Analogous cases occur with the different modifications of phosphorus and selenium.

In reference to chemical changes, it seems probable that sulphide of calcium &c. may exist in two modifications A and B, one stable, the other unstable*. The modification A is transformed into the modification B by the absorption of certain rays, which are gradually given out again as light whilst the substance changes back again into the modification A; the greater the number of molecules of B in the unit volume, the greater the number which pass back again into the modification A in the unit time.

Since the brightness of the phosphorescence increases with the intensity or duration of illumination, the number of molecules of A which have been transformed into B must have been greater, and consequently more molecules of B afterwards are retransformed into A with evolution of light. The decrease in brightness in the same series of experiments must therefore take place at first rapidly, and afterwards more and more slowly, since the number of molecules of B present becomes continually less. Since the substances phosphoresce already during illumination, the retransformation must take place even during the illumination.

With a steady constant illumination, a limiting condition must be reached in which the number of molecules of the condition B, which change back into the condition A, is equal to the number of molecules of B freshly formed. As long as the duration of illumination is less than that required to bring

* It is not impossible, but probable, that Balmain's luminous paint consists of a mixture of substances of which each exists in two modifications. Our theories remain then the same.

about the limiting condition, an increase in the duration produces an increase in the number of transformed molecules. The evolution of light dependent upon the transformation of molecules B into molecules A will therefore last longer.

We have thus an exactly analogous case to dissociation under the influence of heat, which *appears* to come to an end at a definite condition of equilibrium, the condition of which is that the number of molecules decomposed is equal to the number of molecules formed. As an increase and decrease of incident light act upon phosphorescent substances, so a rise and fall in temperature act in this case. We may therefore apply the conclusions which hold good in the phenomena of dissociation to our case.

The results of experiment agree with these theoretical conclusions, and in the first case the way in which the experimental results run.

If x is the quantity of the modification B in the unit of volume, the equation holds good,

$$-i = c \frac{dx}{dt} = -acx;$$

where i is the brightness, and a and c are constants. c gives the brightness produced by the transformation of the quantity 1 in the unit time, whilst a , *ceteris paribus*, determines the rapidity of the transformation.

By integration we obtain $x = Ce^{-at}$, where C is an integration-constant, which is proportional to the initial intensity. It is itself dependent upon the number of molecules of A which are, upon the whole, transformed into B at the time $t=0$; then

$$-i = c \frac{dx}{dt} = -acCe^{-at};$$

the brightness must therefore vary according to an exponential function, which is nearly the case.

The quantity acC denotes the brightness existing at the time $t=0$. For two different series of experiments only the constant C changes, and assumes perhaps the values C_1 and C_2 . The ratio of the intensities i_1 and i_2 , at any equal times t , is therefore

$$\frac{i_1}{i_2} = \frac{C_1}{C_2} \frac{ac}{ac};$$

consequently equal to that at the time zero, and therefore constant.

If we observe at any time for two series of experiments brightnesses which have the ratio $p : 1$, then in the first case

p times as many molecules B are changed into A, as in the second case.

This must consequently also be the case at all subsequent equal times. The brightnesses i_1 and i_2 observed at equal times in the two series of experiments must therefore in fact always stand to each other in the same ratio.

If we obtain by interpolation, from the data given above, the brightnesses i_1 and i_2 for the times t in the series of experiments I. and II., we obtain the values of i_2/i_1 given in the following Table:—

t	4".	15".	25".	40".	1'.
i_1	27.8	13.3	6.2	4.2	3.2
i_2	16.8	86.5	44.7	20.5	14.8
i_2/i_1	6.0	6.5	7.2	5.0	4.7

The ratio i_2/i_1 is as constant as can be expected with such necessarily difficult observations. Whilst the brightness sinks to $\frac{1}{9}$ of the original value, the ratio i_2/i_1 varies between 6.0, 7.2, and 4.7. When the brightness becomes small, the difficulties of the observation increase, and consequently the uncertainty of the determination.

From this theory, that the transformation of a modification B into another modification A produces the phosphorescence, we have also the explanation of the fact that particular rays of the spectrum produce a phosphorescence more vivid, but of shorter duration. It is evidently the rays absorbed by the modification B which, because they excite the molecules of B, accelerate their transformation with more active evolution of light.

In this lies the explanation of the following phenomenon. M. H. Becquerel* determined, on the one hand, the wave-lengths λ , λ_1 , λ_2 , ... of those rays in the violet which excite phosphorescence in different sulphur compounds of the metals of the alkaline earths of analogous composition, and then the wave-lengths λ , λ_1' , λ_2' , ... in the infra-red of those rays which strengthened it; then the order of the λ 's and of the λ 's was the same, only that for those bodies to which in the violet a greater λ corresponded, a smaller λ' was found in the infra-red. The modifications $B_1 \dots B_n$ will of course be similarly marked for the different bodies, as the modifications of $A_1 \dots A_n$ from which they were formed. The wave-lengths of the absorption-bands of the modifications formed from the different analogous compounds show, however, the same order as the wave-lengths of the absorption-bands of the original substances. Therefore the absorption-bands of the modifica-

* *Comptes Rendus*, xcvi. p. 1853 (1883).

tions A in the violet must show the same order as the modifications B in the infra-red.

The action of very small additions of chromium oxide &c. on the brightness of the phosphorescent light may be explained by their simultaneous action in increasing the sensitiveness. They take up the rays which bring about the modification, and carry them on to the molecules associated with them.

Other phosphorescent phenomena also may be connected with such molecular transformations, such as the triboluminescence of the ketones prepared by Prof. Krafft*, *e. g.* pentadecylparatolylketone &c., which the discoverer was so good as to hand to me for investigation. When these substances are rubbed or broken, there is a remarkably powerful evolution of light.

In the triboluminescence of these bodies we have to do first of all with phosphorescence, *i. e.* with a tolerably slow after-luminosity. If a fragment of the substance be broken in the dark, and moved quickly through the air, a line of light of considerable length is seen. The ketones also shine brightly when examined in the phosphoscope. They may further occur in two modifications—one stable at high temperatures, the other at low temperatures. Under the influence of the heat produced by friction or under the influence of light, as the case may be, there is formed a very small quantity of the modification unstable at low temperatures but stable at high temperatures, which then, after the modifying influences have ceased to act, changes again into the stable form with evolution of light. But if such a transformation produces luminosity, the triboluminescence of these bodies is a chemiluminescence, as is also the phosphorescence which is seen.

The occurrence of luminosity is in this case very interesting, since it is here a well-defined body which phosphoresces and not a mixture as with the compounds of calcium sulphide.

36. In connexion with these measurements I have endeavoured to determine *the ratio of the energy radiated in the phosphorescence of Balmain's luminous paint to the energy received which produces the phosphorescence.* Both energies may be measured by the corresponding brightnesses if we take account of the different physiological action of the colours. In consequence of this very similar colour-impression of the exciting and excited light, they can be compared with the blue comparison-star of Zöllner's photometer, produced by the same method. The measurements, which were carried out with the arrangement described in § 35, are divided into

* *Chem. Ber.* xxi. p. 2265 (1888); *Beibl.* xiii. p. 19.

two parts:—(1) measurement of the energy received, and (2) measurement of the energy radiated.

(1) The brightness of the incident light was obtained by measuring the fraction of it, which, after reflexion from a plane glass plate *G* at an angle of 45° , reached the photometer, after having been sufficiently weakened by passing through interposed smoked glasses.

The fraction x reflected from the glass plate (which is so thick that only the reflexion at the front surface has to be taken into account) can be calculated from Fresnel's formula:

$$x = \frac{1}{2} \frac{\sin^2(i-r)}{\sin^2(i+r)} + \frac{1}{2} \frac{\tan^2(i-r)}{\tan^2(i+r)},$$

where i denotes the angle of incidence, and r the angle of refraction.

We take the index of refraction of the glass plate at 1.5, the angle of incidence 45° , then $x = \frac{1}{20}$, so that of the total incident light $\frac{1}{20}$ reaches the observing-telescope. The weakening by the smoked glasses amounts to $\frac{1}{15000}$. Consequently only $\frac{1}{300000}$ of the total energy incident upon the glass plate reaches the observing-telescope.

Further, the physiological sensitiveness for the region of the spectrum occupied by the phosphorescent light is about four times as great as that for the exciting light which lies nearer the violet*. The brightness of the latter is therefore in absolute energy four times as great as that of the phosphorescent light for the same impression of brightness.

The reading on the photometer gave for the exciting light $\alpha = 6^\circ.12$; the brightness is proportional to $\sin^2 6^\circ.12 = 0.011$.

The brightness, and consequently also the energy of the exciting rays referred to the comparison lamp in the proper colour is therefore proportional to

$$0.011 \times 300000 \times 4 = 13200.$$

The factor 4 is used to take account of the physiological sensitiveness for the two impressions of colour.

(2) After a tablet placed accurately in the same place as the surface of the glass plate had been illuminated for one second, the following readings for α' were obtained at the times t , from which are calculated the brightnesses proportional to $10^3 \sin^2 \alpha'$.

t	...	6".	20".	46".	1' 10".	2'.	3'.	4'.	7'.	9'.	11'.
α'	...	4.6	2.4	1.7	1.4	1.1	0.8	0.6	0.6	0.5	0.4
$10^3 \sin^2 \alpha'$...	6.43	1.75	0.88	0.60	0.37	0.19	0.11	0.11	0.08	0.05

* Cf. H. Ebert, *Wied. Ann.* xxxiii. p. 154 (1888).

In order to find the total emitted brightness falling upon the photometer, a curve was drawn with the times as abscissæ, and the observed brightnesses $10^3 \sin^2 \alpha$ as ordinates. The energy emitted during the time of illumination of a second may be neglected. The area enclosed by the curve, the commencing coordinate, and axis of abscissæ, divided by 10^3 , gives the total brightness radiated. In this way it was shown that when the total evolution of light was compressed into 1 second, and whilst it remained constant, it would amount to 1.7 referred to the comparison lamp. The question now was to determine the total brightness excited from the observed brightness which corresponded to the energy of the rays emitted in a definite direction, viz. towards the telescope, which indeed corresponds to the energy of the rays emitted in all directions.

In fig. 5 let s be the slit through which the light falls upon the plate of Balmain's luminous paint, B ; ω the opening of the screen so often mentioned, O the opening of the objective, e the distance from ω , ϵ the distance of the plate B from ω . The distance of B and ω , and the breadth of the slit were so chosen that rays from the phosphorescing surface should certainly reach all points of the objective O . In order to show this, a narrow strip of looking-glass σ inclined at an angle of 45° to e was brought in front of the objective, and the observer satisfied himself that the image of the diaphragm which it reflected in the direction of the arrow was equally bright, whether the strip was in the middle of the objective or at its edge.

The position of the Balmain's tablet at an angle of 45° compensates the diminished excitation of each separate point of the tablet in consequence of its inclination to the exciting rays by the increased magnitude of the radiating surface, because of its equally great inclination to the rays going to the objective, if we assume that the radiating substance is perfectly transparent to the phosphorescent light.

The cone which has for vertex a point in the aperture of the diaphragm ω , and for base the objective O , cuts out a portion β of the surface of the Balmain's luminous paint, and from the projection of this upon the plane at right angles to the axis of the cone, a portion $\mu = O\epsilon^2/e^2$, which is to be put instead of the surface β , sending its rays into the objective.

In our particular case the radius of the objective O is 19 millim., $e = 180$ millim., $\epsilon = 6$ millim.; consequently

$$\mu = \frac{6^2 \cdot \pi \cdot (19)^2}{(180)^2} = 1.26 \text{ square millimetres.}$$

The radius of the aperture in the screen ω is 0.15 millim., consequently its section is $\pi \times (0.15)^2 = 0.0706$ sq. millim.

If the surface B were simply a reflecting surface, as in the first experiments, then, since the rays from s fall parallel upon B, only a pencil of parallel rays of section ω would reach the objective in the direction of the axis. But since the surface B is self-luminous, rays reach the objective through ω from the whole surface β ; hence the ratio of the radiating surfaces in the two cases is μ/ω (the projection μ is, as we have seen above, to be used instead of β).

Of the rays radiated by each point of the surface B in all directions, only those which pass through the opening ω reach the objective. The observed brightness is therefore

$$\frac{\omega}{4\pi\epsilon^2} = \frac{1}{6400} \text{ of the total radiation;}$$

that is, it bears the same proportion to the total radiation as the surface ω to the surface of the cone with radius ϵ .

The observed brightness of the phosphorescent light was 1.7; hence the total brightness was

$$T = 1.7 \times 6400 = 10880.$$

If light from the whole surface β reached the objective also in the case of mirror-reflexion, the brightness would be μ/ω greater, and a quantity of light corresponding to this latter brightness produces the phosphorescent light.

We had observed a brightness of the exciting light of 13200 units; the quantity of light R, which has really excited the surface β , is, however,

$$R = (1.26/0.0706) \times 13200 = 235000.$$

Of the incident energies R, therefore, we have transformed into radiant energy

$$T/R = 10880/235000 = 0.046,$$

or, in round numbers, $\frac{1}{22}$ of the total incident energy is converted into emitted light.

In this, however, it is assumed in the first place that the phosphorescent substance is transparent for the rays which it emits, and further that the whole of the incident light penetrates into it, whereas no doubt a part is lost by diffusion. Hence the ratio T/R becomes greater.

A somewhat large portion of the energy of the incident and phosphorescence-exciting light is therefore transformed into the energy of emitted light; the remainder has either been absorbed as heat or does not appear again as light in the motions which occur in the rearrangement of the molecules.

Methods of determining the Quantity b.

37. Although the phenomena of phosphorescence in Balmain's luminous paint led to no result in the determination of the quantity b , there are a series of other phenomena which render possible a conclusion as to its magnitude. The results to be thus obtained are brought together in the following. The value of b finally given in each case is in round numbers, since the question can only be as to its order of magnitude. We make use of the equation on p. 337.

$$b = \log \left(\frac{J_i}{J_t} \right) / (t \log e).$$

(a) The observations of interference-bands with a large difference by Michelson and Morley* show that in the case of sodium, bands are visible with a difference of 200,000 wavelengths; we may therefore conclude that after the time which corresponds to the performance of 200,000 vibrations in the case of sodium light, the energy of the vibrating molecule has not yet sunk to $\frac{1}{2}$; for otherwise the difference in intensity of the two interfering pencils of rays emitted at the beginning and end of this time would be too great for the occurrence of still visible interference. That a great number of oscillations may take place undisturbed and without great decrease in intensity is shown by the occurrence of sharp lines in the spectrum, since the vibrations which cause them can only occur upon the free path between two impacts, for so long as the vibrating molecules occupy their opposed spheres of action irregular vibrations are produced. From the experiments of H. Ebert† it follows that the increase in number of luminous particles in the unit volume is almost the only cause of the widening of lines, a sure sign that the modes of vibration become more complicated in consequence of the impacts of molecules of the same kind.

If we take the wave-length of sodium as 0.036 millim., the oscillation period as $1/(5 \times 10^{14})$ seconds, we have the following equation:—

$$J_t = \frac{1}{2} J_0 = J_0 e^{-b \frac{200000}{5 \times 10^{14}}} = J_0 e^{-4 \times 10^{-10} b}; \text{ hence } b = 1.74 \times 10^9.$$

The actual value is no doubt smaller, since no doubt more than 200,000 vibrations occur without disturbance.

(b) Herr Feddersen‡ has observed oscillating discharges

* Michelson and Ed. Morley, *Sill. Journ.* [3] xxxiv. p. 421 (1887).

† Wied. *Ann.* xxxiv. p. 85 (1888).

‡ Pogg. *Ann.* cxvi. p. 132 (1862).

in a rotating mirror, which were separated by perfectly dark intervals. The duration of the oscillations was 100×10^{-7} sec. The dark interval which corresponded to the decrease from a high intensity to a very small intensity, let us say to about $\frac{1}{10000}$ of the original value, amounted to about $\frac{1}{10}$ of the oscillation period, *i. e.* 10^{-6} sec.

Hence

$$J = \frac{J_0}{10000} = J_0 e^{-b \times 10^{-5}}, \quad b = 10^7.$$

The true value is no doubt greater.

(c) Electric discharges in Geissler's tubes often appear so close together in a rotating mirror that they follow each other at intervals of $\frac{1}{10000}$ second, and yet they are separated by almost absolutely dark intervals. We may here assume that during $\frac{1}{100000}$ second the brightness has not sunk more than to $\frac{1}{10}$, and then we obtain

$$J_t = \frac{J_0}{10} = J_0 e^{-b \times 10^{-5}}, \quad b = 2.3 \times 10^5.$$

(d) In the phosphoroscope fluorescent liquids do not shine after a longer interval than $\frac{1}{4000}$ seconds. We may, therefore, put here

$$J_t = \frac{J_0}{100} = J_0 e^{-b \times 10^{-4}}, \quad \text{or } b = 5 \times 10^4.$$

b is, however, certainly greater. Its value cannot, as yet, be determined more accurately by this method, since we are not as yet acquainted with any phosphorescent liquids, and can therefore only give an upper limit for t . In the meantime the magnitude of b may also be determined for pure solutions of fluorescent substances in the following manner:—We determine it for solutions of any fluorescent substance, eosin, in solid gelatine, and then in gelatine containing increasing quantities of glycerine or water, which, as I have shown, phosphoresce distinctly. By extrapolation we obtain the value for an eosin solution in glycerine without addition of gelatine. Since in winter the sun's rays, necessary for these measurements, hardly fall upon the Institute in Erlangen, I hope to report later as to numerical data in continuing the investigations upon fluorescence and phosphorescence. The interferences observed with fluorescent bodies with large differences of path cannot be used, like the similar phenomena in gases, for the determination of b . In gases each impact produces luminous motions, which die away upon the free path, but with constantly illumi-

nated bodies there is always a portion of the radiated luminous energy again replaced by the absorbed vibrations of light.

(e) From determinations with solid phosphorescent bodies, such as uranium nitrate, b is found to be about 10^3 . We will treat this more fully in § 41. Values of the same order of magnitude result from observations with other phosphorescent bodies, in which no chemical transformations can be assumed.

(f) From the time necessary for a platinum wire of 0.26 millim. thickness to cool from a bright white heat to darkness, viz. 8 seconds, we obtain about

$$b=100.$$

The time taken as the basis of this calculation is, without doubt, too great, since a portion of the radiated luminous energy is again replaced by motions of translation. The value of b is therefore greater.

Evaluation of the Store of Luminous Energy.

38. The following values, in gramme-calories, for the store of luminous energy L were obtained according to the equation $L=E_0/b$, where E_0 refers to the unit weight.

We had found (§§ 26 and 21) for

$$\text{Sodium} \quad . \quad . \quad . \quad E_0=3.2 \times 10^3,$$

$$\text{Platinum} \quad . \quad . \quad . \quad E_0=2.2 \times 10^4.$$

If now we put for both substances,

$$b=10^8,$$

then the store of luminous energy is for

$$\text{Sodium} \quad . \quad . \quad . \quad L=3.2 \times 10^{-5} \text{ gramme-calories,}$$

$$\text{Platinum} \quad . \quad . \quad . \quad L=2.2 \times 10^{-4} \quad , \quad ,$$

If we put $b=10^3$ for platinum, which may be nearer the truth, since we have here to do with a solid body, then the store of luminous energy for platinum is

$$L=22 \text{ gramme-calories.}$$

These numbers represent in calorimetric units, as explained above, the actual kinetic energy of the intra-molecular motions which give rise to the emission of light under the above conditions of luminosity. We thus obtain for the first time a reliable insight into the order of magnitude of the energies of these motions, and are thus in a position to obtain clearer notions about these motions themselves.

Proof that the Material Molecules are the Carriers of the Store of Luminous Energy. Application to Spectra.

39. We will now investigate whether the æthereal envelopes

of the molecules in their material portions perform the motions which correspond to the store of luminous energy.

The store of luminous energy is emitted as light in the course of time. It is originally present in the form of vibrations whose maximum *vis viva* is exactly equal to this store of energy. The assumption that it is present in the first instance as potential energy, depending upon rearrangements of the atoms and molecules, presents great difficulties, especially in the case of the monatomic gases.

If we denote by v the velocity with which the vibrating molecules pass through the position of equilibrium, by a the amplitude, and by T the period of oscillation, then, as is known, v is given by the maximum value of

$$\frac{dx}{dt} = \frac{d}{dt} \left(a \sin 2\pi \frac{t}{T} \right) = \frac{2\pi a}{T} \cos 2\pi \frac{t}{T},$$

i. e.

$$v = \frac{2\pi a}{T}.$$

If m is the mass of the vibrating molecule, then $\frac{1}{2}mv^2$ is its store of luminous energy. We must introduce into the equation the maximum value of v , and not, as in the calculation of intensity, the mean value during a vibration; just as the energy involved in the vibrations of a pendulum is determined by $\frac{1}{2}mv^2$, where again v denotes the value of the velocity with which the pendulum passes through its position of equilibrium. The total energy determined by the oscillatory motion is gradually radiated, quite independently of the fact that it is made up of potential and kinetic energy, at other times than those when the particle is passing through its position of equilibrium.

If, further, G is the weight of the vibrating mass which is contained in 1 gramme of the luminous body, γ the acceleration due to gravity, 430 the mechanical equivalent of heat, then

$$430 \frac{E_0}{b} = \frac{1}{2} \frac{Gv^2}{\gamma} = \frac{1}{2} \frac{G}{\gamma} \frac{2^2\pi^2 a^2}{T^2}.$$

We will put $\gamma = 10$ metres, $T = 1/(a \times 10^{14})$, where $a \times 10^{14}$ denotes the number of vibrations in æther for the ray of light in question; then

$$a^2 = \frac{2 \times 10 \times 430}{2^2 \times \pi^2 \times 10^{28}} \frac{E_0}{a^2 G \times b} = \frac{1}{4 \cdot 6 \times 10^{25}} \frac{E_0}{a^2 G b} (\text{metre})^2.$$

In reference to the weight G of the vibrating particle we may make two extreme assumptions:—

(1) We may assume that it is the material particles themselves which vibrate and thus cause the emission of light; in this case $G=1$.

(2) We may assume that it is the luminiferous æther surrounding the molecule which vibrates. The weight of the vibrating luminiferous æther may be found as follows:—The weight of a molecule of hydrogen* is, according to the kinetic theory of gases, 15×10^{-23} gr., its volume 4×10^{-25} cub. cent. Now the density of luminiferous æther in reference to water is 10^{-17} ; the weight of the æther contained in the volume of a hydrogen molecule is therefore of the order of magnitude

$$4 \times 10^{-25} 10^{-17} = 4 \times 10^{-42} \text{ gramme};$$

its weight is therefore $4 \times 10^{-42} / (15 \times 10^{-23}) = 2.6 \times 10^{-20}$ that of the hydrogen molecule. Therefore in this case

$$G = 2.6 \times 10^{-20} \text{ gr.}$$

In this we make the simplest assumption for the calculation, that the luminiferous æther occupies the whole volume of the material molecule, and further that it possesses the same density in the bodies as *in vacuo*. If we should allow the density of the æther to change in accordance with the indices of refraction, or form only an envelope surrounding the molecule, the order of magnitude of the values obtained would not be altered.

(1) Vibrating material particles,

$$a_K^2 = \frac{1}{4.6 \times 10^{25}} \frac{E_0}{\alpha^2 b} (\text{metre})^2.$$

(2) Vibrating æther particles,

$$a_A^2 = \frac{1}{4.6 \times 10^{25}} \frac{E_0}{\alpha^2 b \times 2.6 \times 10^{-20}} (\text{metre})^2 = \frac{1}{1.3 \times 10^6} \frac{E_0}{\alpha^2 b} (\text{metre})^2.$$

We will now determine the magnitudes a_K and a_A for sodium and platinum.

For sodium,

$$E_0 = 3.2 \times 10^3 \text{ gr. cal.}, \quad b = 10^8, \quad \alpha = 5;$$

whence it follows, if we now put millimetres instead of metres,

$$a_K = 1.7 \times 10^{-13} \text{ millim.}$$

$$a_A = 1.1 \times 10^{-3} \text{ ,,}$$

For platinum, we obtain, if we put for b the value which it

* Upon the dimensions, weights, &c. of the molecules see R. Rühlmann, *Mechanische Wärmetheorie*, ii. p. 245 (1885).

obtained with phosphorescent solid bodies, and further assume that the platinum emits all its energy in rays situated in the infra-red (we know that the greater portion of the radiation falls there), and put $E_0 = 2.2 \times 10^4$, $b = 10^3$, $\alpha = 2$:—

$$\alpha_K = 3.5 \times 10^{-10} \text{ millim.}$$

$$\alpha_A = 2.1 \quad ,,$$

If here also we put $b = 10^8$, we should have

$$\alpha_K = 1.1 \times 10^{-12} \text{ millim.,}$$

$$\alpha_A = 6.6 \times 10^{-3} \quad ,,$$

If now we compare the values obtained for the amplitudes α_K and α_A with the diameter of the molecules, for which the data of the kinetic theory of gases gives numbers of the order 10^{-7} millim., we obtain the following results.

If in the platinum the store of luminous energy results from vibrations of the luminiferous æther, the amplitude of the æther must amount to $\frac{1}{100}$ millim. or more, which cannot be imagined; consequently the store of luminous energy must reside in the vibrations of the material molecules themselves.

The same holds also for sodium, for here also the amplitudes of vibration of the æther-particles reach magnitudes a thousand times greater than the dimensions of the molecules.

If we assume, on the other hand, that the vibrations of the material molecules are the cause of the store of luminous energy, then the errors in both cases are only small fractions of the diameter of a molecule, which is immeasurably more probable.

For the sake of control let us ask what value b must have so that, in the vibrations of the æther-atmosphere in glowing platinum, the amplitude may be equal to the diameter of the molecule 10^{-10} metre, or 10^{-7} millimetre.

We obtain from the above equation for α_A^2 , if we put $\alpha_A = 10^{-10}$, for platinum,

$$\frac{1}{b} = \frac{(10^{-10})^2 \times 1.3 \times 10^6 \times 2^2}{2.2 \times 10^4} = 2.4 \times 10^{-18},$$

If, again, at the time $t=0$ the intensity is i_0 , then

$$i_t = i_0 e^{-bt}, \quad \log \frac{i_t}{i_0} = -bt.$$

If

$$i_t = \frac{1}{10} i_0, \text{ then } bt = 1,$$

$$t = \frac{1}{b} = 2.4 \times 10^{-18} \text{ seconds ;}$$

consequently at the time $t = 2.4 \times 10^{-18}$ sec. the initial intensity has sunk to $\frac{1}{10}$.

The duration of the vibrations investigated was $\frac{1}{2} 10^{-14}$. Here, then, whilst the light has sunk to $\frac{1}{10}$ of its initial intensity, $2.4 \times 10^{-18} / \frac{1}{2} 10^{-14}$ vibrations, *i. e.* only about $\frac{1}{10000}$ vibration, have been performed, which is quite absurd.

According to an analogous calculation for sodium,

$$\frac{1}{b} = \frac{(10^{-10})^2 1.3 \times 10^3 \times 5^2}{3.2 \times 10^3} = 9 \times 10^{-17}.$$

The brightness $\frac{1}{10}$ would be reached at a time 9×10^{-17} seconds. The duration of a vibration for sodium is $\frac{1}{5} 10^{-14}$ seconds; consequently a decrease of brightness to $\frac{1}{10}$ would have taken place after $9 \times 10^{-17} / \frac{1}{5} 10^{-14}$ vibrations, *i. e.* $4.5/100$ of a vibration.

Thus for sodium as well as for platinum the above assumption leads to quite contradictory results.

The above considerations thus lead to the same result. *The store of luminous energy is dependent upon the vibrations of the material molecules, and not upon those of the luminiferous ether.*

The differences in the spectra of different substances thus depends upon the different constitution of their material molecules and not upon that of the surrounding æther envelopes.

With the luminous atoms, moving freely through space, which yield line-spectra, these vibrations can only consist in opposite changes of position of the constituent parts of a material atom, which therefore can have no absolutely fixed form; they are thus exactly analogous to the vibrations of a deformed sphere. By this proof we obtain a secure basis for those theoretical views which seek to deduce special oscillatory motions for each body from the equations which hold good for elastic bodies.

For the undecomposed luminous compounds (which even with the greatest dispersion yield bands not resolved into lines, p. 376), it is displacements of the separate entire atoms which compose a molecule with reference to each other which call forth luminosity. Since the amplitudes are so very small in comparison with the diameter of a molecule, such vibrations may very well occur without the separate atoms passing beyond their mutual spheres of action, that is without decomposition occurring.

From what has been already shown it follows at once that with the altogether different nature of the forces upon which the vibrations depend, which are in the case of free atoms of the nature of elastic forces, and in the case of molecules

mutual forces between the separate atoms, the respective spectra must be altogether different.

Store of Luminous Energy and Specific Heat of Monatomic Gases.

40. The determination of the store of luminous energy gives us also a means of explaining an otherwise somewhat puzzling circumstance. The experiments of Messrs. Kundt and Warburg* have shown that mercury vapour behaves as to its specific heat as a monatomic gas, *i. e.* that upon heating it is only the velocity of translation of the molecules which increases, but no internal energy is produced in each atom. This result is contradicted by the fact that mercury vapour gives a very fine bright spectrum of lines, which certainly must result from intra-molecular motions. As we do not yet possess the data to calculate the store of luminous energy in mercury, we will employ the results for sodium in order to discover the cause of this apparent contradiction.

Sodium vapour must, as the line-spectrum certainly shows†, behave as a monatomic gas at the temperature of the flame.

If we take the atomic heat of sodium vapour as the same as that of mercury in the gaseous condition, viz. 3.0, then its specific heat is 0.13. The total heat-contents W , which is given to 1 gramme of sodium vapour when heated from the absolute zero of temperature to 1000° , is then 0.13×1273 ,

$$W = 165.5 \text{ cal.}$$

The store of luminous energy L , however, is only

$$L = 3.2 \times 10^{-5} \text{ cal.}$$

i. e.

$$\frac{L}{W} = 2 \times 10^{-7} \text{ about.}$$

The luminous energy is thus an immeasurably small fraction of the total energy, and need not be taken into account in observations of the specific heat. This would also be true if, as is the case, separate sodium lines existed in the infra-red.

The conditions are otherwise if we compare the quantities W and L for glowing platinum.

If we put the mean specific heat of platinum between

* Pogg. *Ann.* cliv. p. 353 (1875).

† Cf. H. v. Helmholtz in a work by Moser, Pogg. *Ann.* clx. p. 182 (1877), and E. Wiedemann, Wied. *Ann.* v. p. 501 (1878).

—273° C. and 1000° as somewhat less than between 0° and 1000° C. (see above) at 0.037, then

$$W = 1273 \times 0.037 = 47.1.$$

For L we have found two values for platinum, $L = 2.3 \times 10^{-4}$ and 23, whence we obtain the two values

$$\frac{L}{W} = 4.88 \times 10^{-6}, \quad \frac{L}{W} = 4.88 \times 10^{-1}.$$

The luminous energy forms here also in the first case only a small fraction of the total energy, in the second case one much larger. It is indeed not impossible that in solid bodies the motions of translation of the centre of gravity, which here consist in vibrations about the position of equilibrium of the molecule, give rise to the production of light-vibrations. That the centre of gravity cannot be greatly displaced from its mean position of equilibrium follows from the occurrence of interference-bands with large differences of path with solid bodies*.

Store of Luminous Energy in Photoluminescent Substances.

41. We have already, § 37(e), indicated the possibility of a more exact determination of the quantity b in certain cases, *e.g.* in uranium nitrate. Such a determination is in fact possible with photophosphorescent substances. To take in this case as the basis of our calculation about the same intensity as initial intensity, which the phosphorescent substance possesses at any time, might appear scarcely reasonable even if the conditions of excitation were exactly determined, since it depends upon the construction of the particular phosphoroscope used, the size of the openings, and of the dark space between, &c.

We must rather determine the brightness which the body in question has upon constant illumination, corresponding to the brightness which the light commonly called fluorescent light shows. This brightness is solely dependent upon the property possessed by the body of utilizing the vibrations of incident light for the production of luminous energy, and the rapidity with which it dies away.

We have seen that the brightness of the light upon constant illumination possesses the value

$$i_f = \frac{AJ}{b},$$

* E. Wiedemann, *Verh. der Phys.-med. Soc. in Erlangen*, July 1887.

where J is the brightness of the incident light, and A a constant dependent upon the nature of the body.

The brightness determined in a phosphoroscope of the former construction* is

$$i_{\delta} = \frac{AJ}{b} (1 - e^{-b\mu}) \frac{1 - e^{-b\tau}}{1 - e^{-b\tau}e^{-b\tau'}e^{-b\delta}}.$$

τ is the time during which the body is illuminated, τ' the time during which it is not illuminated, δ the time between the end of the illumination and the beginning of the observation, μ the time during which the body is observed.

In the apparatus which I used,

$$\tau = \delta, \tau' = 3\delta, \tau + \tau' = 4\delta, \mu = \delta,$$

then

$$i_{\delta} = \frac{AJ}{b} \frac{(1 - e^{-b\delta})^2}{(1 - e^{-4b\delta})} \times e^{-b\delta} = \frac{AJ}{b} f,$$

where

$$f = \frac{(1 - e^{-b\delta})^2}{(1 - e^{-4b\delta})} e^{-b\delta}$$

represents the constant independent of the velocity of rotation.

I will consider later on the properties of the function f , which exhibits a maximum value. We see at once that with decreasing velocity of rotation, that is with increasing δ , the function f first increases and reaches a maximum for a value of $b\delta = 1.5$ about, and then diminishes, at first rapidly and afterwards more slowly. The smaller b is, the greater is δ when the maximum occurs. If b is very small, *i. e.* if the decrease in intensity takes place very slowly, the brightness appears scarcely altered with great changes in the velocity of rotation. One would also come to the same conclusion as to the existence of such a maximum of brightness by simply considering the nature of the excitation of light and the expenditure of light.

In order to find b we determine the intensities i_{δ} for a series of different velocities of rotation, *i. e.* for different values of δ . From the values obtained from each two measurements we calculate b , and thence for a definite value of δ the factor f . By dividing the value of f into the corresponding value of i_{δ} we obtain the value of the intensity of the fluorescent light $F : i_f = AJ/b$.†

* E. Wiedemann, Wied. *Ann.* xxxiv. p. 458 (1888).

† By observations of the phosphorescent light with different velocities of rotation, and by a similar line of reasoning, it is not in general possible to determine the brightness of the fluorescent light of crystals &c. Whilst with liquids and the different sorts of glass it is possible, at least to a very great extent, by forming them with plane surfaces, to reflect the

The following experiments have for their object in the first place only to establish the order of magnitude of the quantity b . I shall communicate further results in the continuation of the investigation on phosphorescence and fluorescence.

The divergent rays issuing from the phosphorescent body, which is covered by a screen perforated by a small aperture a (fig. 6), are rendered parallel by the lens L ; they fall then upon the prism P , and are united by the objective O of a Zöllner's astrophotometer to a real spectrum.

A plate of uranium nitrate 0.1 millim. thick was employed for the experiments, which was illuminated by the rays from a Schuckert's arc lamp concentrated by a lens.

A determination of the specific gravity of the uranium nitrate employed in benzol gave the number 2.5.

From the experiments with the phosphoroscope, with different velocities of rotation, for which δ lay between 0.0004 and 0.008 second, we obtain by the use of the above formula

$$b = 2 \times 10^3,$$

a number which agrees as to order of magnitude with the value found by Becquerel.

The spectrum of the phosphorescent light was found, in agreement with the statements of other investigators, to consist of a series of bright bands divided by dark spaces. The comparison-star was coloured green, and the brightness was determined for the three green bands in the phosphorescent light when the disk of the phosphoroscope made 124 revolutions per second. The readings α on the Nicol of the Zöllner's photometer, when the brightnesses are proportional to $\sin^2 \alpha$, were:—for the green band $\alpha_1 = 19^\circ$, for the yellowish-green $\alpha_2 = 16^\circ$, and for the blue-green $\alpha_3 = 14^\circ$. The red and blue were only weak, and too different in colour from the comparison-star to render the comparison possible.

Then the uranium nitrate was removed, and placed before the small diaphragm of the amyliacetate lamp. The photometer was read at 45° for the point in question in the yellowish

exciting light in such directions that it does not enter the eye (or photometer), so rendering the determination of the brightness of the usually very feeble phosphorescent light more difficult; this is not possible with small crystals or powders. On the other hand, two determinations of brightness at two different velocities of rotation formed, as we have shown, an excellent means of finding the brightness sought, at least very nearly. The predominance of the diffusely reflected light is in fact the reason why many bodies shine very brightly in the phosphoroscope, whereas otherwise they require very careful examination to detect an emission of light.

green, after interpolating another glass which reduced the brightness to $\frac{1}{34}$.

But now we see the comparison-lamp constantly, but the phosphorescent light only during $\frac{1}{4}$ of a revolution. The actual brightness of the latter is therefore four times as great as the observed apparent brightness. For the yellowish-green band, the brightness F of the fluorescent light, according to the previous determination of f , denotes the above-mentioned factor, and "const." a constant peculiar to the photometer,

$$F = \frac{4 \times \sin^2 16^\circ \times \text{const.}}{f}.$$

Since the disk makes 124 revolutions per second, and $\frac{1}{16}$ of the circle is covered by each opening, $\delta = \frac{1}{2000}$; further $b = 2 \times 10^3$, whence it follows that $f = 0.15$, so that

$$F = 2.0 \text{ const.}$$

The brightness of the amyacetate lamp is Y :

$$Y = 34 \times \sin^2 45^\circ \times \text{const.} = 17 \text{ const.}$$

Hence in the yellowish-green the brightness of the fluorescent light of our uranium plate under these conditions of illumination is

$$\frac{F}{Y} = \frac{\text{const. } 2}{\text{const. } 17} = 0.12$$

times as great as that of the amyacetate lamp.

In the spectrum of the phosphorescent light, the bands are so distributed that they occupy about $\frac{1}{4}$ of the total spectrum between the Fraunhofer-lines C and F. They are, however, not equally bright. If we take account of their difference in brightness, we may assume with considerable probability that they would occupy $\frac{1}{8}$ of a spectrum that should have everywhere a brightness corresponding to that of the green line.

According to this reasoning, $\frac{1}{8} \times 0.12 = 0.015$ of the energy of the light of the amyacetate lamp is included in the phosphorescent light between C and F.

We may further assume with sufficient exactness for our purpose, that the distribution of brightness in the part of the spectrum of the amyacetate lamp used and in that of the glowing platinum wire is the same. For the latter calculations, exactly similar to the former ones of § 24, give for the quantity of energy between C and F 0.016 of the total energy.

The energy of the amyacetate lamp for a region within the visible spectrum (p. 259) is

$$E' = 0.13 E,$$

where E denotes the energy emitted by the unit of area (1 square centim.) of platinum. In the region between C and F , a quantity of energy, 0.016×4.7 gr. cal. per second, is radiated from the platinum ; so that in this region

$$E' = 0.13 \times 0.016 \times 4.7 = 0.0098 \text{ gr. cal. per sec.}$$

In fluorescent light, such as would be radiated in all directions by a crystal of uranium nitrate whose surface is 1 square centim., we find the energy

$$F = 0.015 E' = 1.47 \times 10^{-4} \text{ gr. cal. per sec.}$$

The thickness of the crystal investigated was 0.1 millim., its specific gravity 2.5. The luminous mass, which has 1 square centim. surface, is

$$\frac{2.5}{100} = 2.5 \times 10^{-2} \text{ gr.}$$

Therefore in 1 second there is radiated from 1 gr. a quantity of energy

$$J_0 = \frac{1.47 \times 10^{-4}}{2.5 \times 10^{-2}} = 5.9 \times 10^{-3} \text{ gr. calories.}$$

We had found above for the quantity $b \ 2 \times 10^3$, so that the store of luminous energy per gramme and the conditions becomes

$$L = \frac{J_0}{b} = \frac{5.9 \times 10^{-3}}{2 \times 10^3} = 2.9 \times 10^{-6} \text{ gr. calories,}$$

if we take the initial intensity to be $F=i$, as measured in energy.

This quantity of energy of a purely luminescent motion agrees, as to order of magnitude, with that found (§ 38) for the sodium rendered luminous in a Bunsen flame. When heated from the absolute zero to 0°C. , 1 gr. uranium nitrate (if we take its specific heat as 0.5) would receive a quantity of heat equal to 137 gr. calories ; in comparison with this the store of luminous energy disappears altogether.

The further discussion of these and other numbers shows that the temperature of luminescence of the uranium nitrate examined is very high. The considerations, as well as a more accurate determination of particular values, must be reserved for a later communication.

I desire here to express my best thanks to Dr. Ebert, who has most kindly assisted me with these investigations, both in the measurements and in the calculations.

Erlangen, December 1888.

XLVI. *Note on Elementary Nomenclature in Geometrical Optics.*

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

THE notation of the paper, "Notes on Geometrical Optics," by Professor Silvanus P. Thompson, in the *Philosophical Magazine* for October, is in many respects simpler than that used in current text-books. Sir W. Thomson, in lecturing to the Natural Philosophy Class in this University, uses a nomenclature which, so far as I know, has not yet been published. I have his permission to give the following summary of it, which is virtually a copy of a cyclostyled paper that is put into the hands of each student when Sir W. Thomson commences his lectures on Optics.

Nomenclature.

(1) The *refractivity* of a substance is the difference between the index of refraction of the substance and unity.

(2) The *potency* of a lens depends on two factors, refractivity and curvature. It is equal to the product of the refractivity into the algebraic sum of the curvatures of the lens. The *potency* of a lens is called *convergivity* when it is for convergence, and *divergivity* when it is for divergence.

(3) The *convergence* or *divergence* of a pencil of light is the reciprocal of the distance of the source, or of the image of the source, from the centre of the lens.

(4) Either convergence or divergence is altered by addition or subtraction of the potency.

(5) *Convergence* of a pencil of light after passing through lens = convergence of incident pencil + convergivity of lens.
Divergence of a pencil of light after passing through lens = divergence of incident pencil - convergivity of lens, or = divergence of incident pencil + divergivity of lens.

Notation.

(1') Refractivity = $(\mu - 1)$, where μ is the index of refraction of the substance.

(2') For a double convex lens, as in fig. 1,

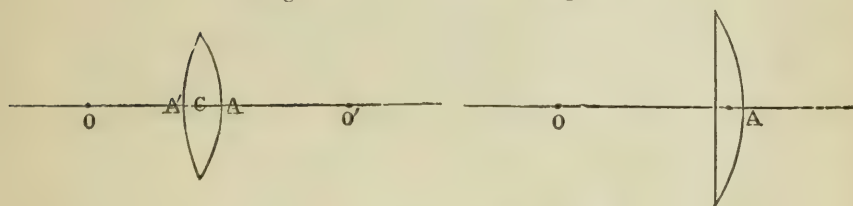
$$\text{Convergivity} = (\mu - 1) \left(\frac{1}{r} + \frac{1}{r'} \right).$$

For a plano-convex lens, as in fig. 2,

$$\text{Convergivity} = (\mu - 1) \frac{1}{r}; \quad \frac{1}{r'} = 0.$$

Fig. 1.

Fig. 2.

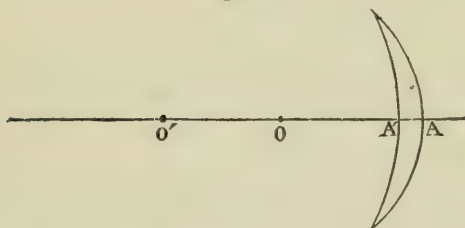


$OA = r$; $O'A' = r'$ in all the figures.

For a concavo-convex lens, as in fig. 3,

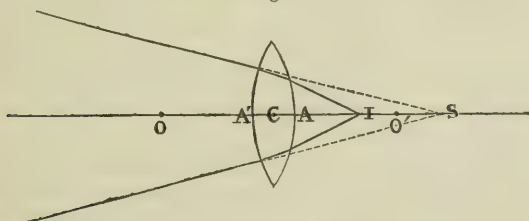
$$\text{Convergivity} = (\mu - 1) \left(\frac{1}{r} - \frac{1}{r'} \right); \quad r' > r.$$

Fig. 3.



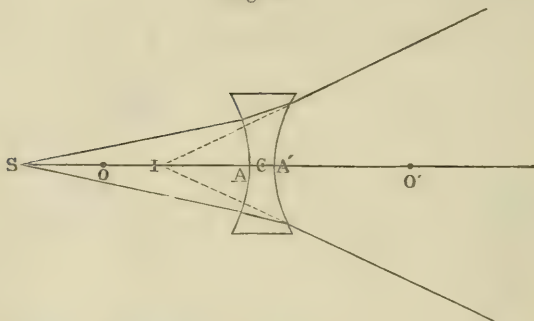
And similar formulæ for the three diverging lenses.

Fig. 4.



(3') The convergence of the pencil of light in fig. 4, and the divergence of the pencil of light in fig. 5 = $1/SC$.

Fig. 5.



(5') In fig. 4 and also in fig. 5,

$$\frac{1}{IC} = \frac{1}{SC} + \frac{1}{f},$$

where f is the focal length of the lens.

I am, &c.,

MAGNUS MACLEAN.

Physical Laboratory, The University,
Glasgow, October 16, 1889.

XLVII. *The Constitution of the Aromatic Nucleus.* By S. A. SWORN, B.A., Assoc. R.C.Sc.I., late Brakenbury Scholar of Balliol College, Oxford.*

ONE of the most important developments of theoretical chemistry in recent times has been the view that symmetry plays an important part in the aggregation of those atoms which, when combined together, form the fundamental molecular units of organic chemistry. Van't Hoff, in the case of the derivatives of marsh-gas, has brought forward views which have received much, and for the most part favourable, criticism. On the other hand the views of R. Meyer (*Ber.* xv. p. 1823) and of J. Thomsen (*Ber.* xix. p. 2944), who have each proposed a symmetrical formula for benzene, have been less favourably received.

It is a remarkable fact that closed chains consisting of six carbon atoms (never five or seven atoms) are produced by the action of dehydrating agents upon such bodies as acetone, and by other condensations. Observations on the specific volumes of aromatic compounds further show that these closed chains are characterized by a compactness of molecular

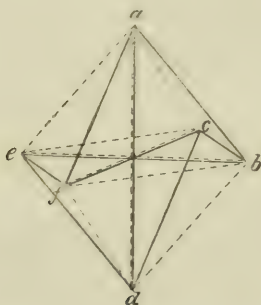
* Communicated by the Author.

structure unknown in other compounds. This view is supported by the fact that the heat of combustion of benzene is much less than that of dipropargyl. The persistency with which the aromatic nucleus holds together throughout a long series of chemical changes leads us to the same conclusion.

These considerations led the author to suppose that six carbon atoms have attained to their most stable condition of equilibrium only when they have taken up the most symmetrical positions in space, viz. those at the points of a regular octahedron. Two such symbols have been proposed. That of Meyer is a modification of Ladenburg's prism formula. That of Thomsen is an adaptation of the diagonal symbol of Claus, and a special case of Koerner's glyptic symbol.

The symbols of Thomsen and Meyer are complementary to each other. The ortho-linkages of the one and the meta-linkages of the other together form a complete octahedron.

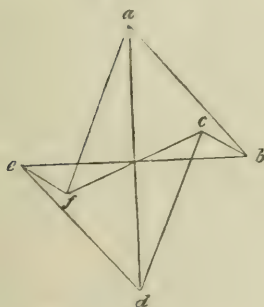
Fig. 1.



Thomsen's symbol (continuous line).

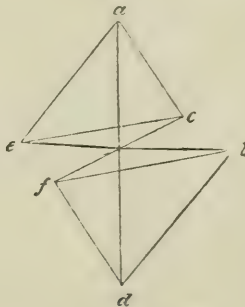
Meyer's symbol (dotted line).

Fig. 2.



Thomsen's symbol.

Fig. 3.



Meyer's symbol.

The two symbols above given are the only octahedral symbols which will account for the relationships of the benzene substitution derivatives. Moreover, these relationships can be explained only on the assumption that the properties of the derivatives in question are dependent both upon the positions in space of the carbon atoms and upon the nature of the atomic interactions.

$$\left. \begin{array}{l} (1-2) \text{ Ortho-} = a, b \text{ \&c.} \\ (1-3) \text{ Meta-} = a, c \text{ \&c.} \\ (1-4) \text{ Para-} = a, d \text{ \&c.} \end{array} \right\} \text{ in each diagram.}$$

The symbol of Kekulé should give rise to four isomeric substitution derivatives when the introduced radicals are similar, and to five when they are dissimilar (Wroblewsky *Ber.* xv. p. 1023). The researches of Wroblewsky on the toluidines (*Annal.* cxcii. p. 196) have proved that only one ortho- and one meta-toluidine exist. (See also Lobry de Bruyn, *J. C. S.* 1885, abstracts, p. 972.) Kekulé has given an explanation of the non-existence of two isomeric ortho-derivatives, which is, however, very unsatisfactory. We shall not discuss this point, because there are so many others which are in conflict with his theory.

It may be pointed out that the angles abc and abe are respectively 60° and 45° , whilst the angle enclosed by any pair of valencies directed from the centre of a regular tetrahedron to its apices is $109^\circ 28'$, and it may therefore be argued that the octahedral formulæ are in direct opposition to the Van't Hoff theory. But Van't Hoff himself states that the tetrahedron is not necessarily regular (see *Dix années dans l'histoire d'une théorie*, p. 27). The author's view of the "tetrahedral theory" involves no arbitrary assumptions as to the nature of chemical affinity or the shape of the atoms. It is briefly as follows:—By means of the forces of chemical affinity the carbon atom is able to unite with other groups. These forces must act in four directions in space, which we may call valency-directions. The directions are dependent upon the nature of the associated groups. Only when they are precisely similar will the valency-directions be perfectly symmetrical.

In the octahedral formula for benzene we have one hydrogen atom on the one side of a plane drawn through a carbon atom a perpendicular to ad (fig. 1). On the other side

of this plane is a system of five carbon and five hydrogen atoms. From this inequality it follows that the valencies of the carbon atom *a*, and similarly of any other of the six carbon atoms, will be unsymmetrically directed. The directions of the valencies of any particular carbon atom are determined, not by the symmetry of the whole molecule about its centre, but by the configuration and mode of attachment of the rest of the molecule about the atom. Such a view is not inconsistent with any of the facts which support the theory of Van't Hoff.

Armstrong has stated (J. C. S. 1888) that in "the symbolic system introduced by Van't Hoff a double bond is represented as the precise equivalent of two, and a treble bond as that of three single bonds; which all observations show is a misrepresentation of the facts." This appears to me to be a misconception, for I have always considered the instability of "unsaturated compounds" to be dependent upon the fact that the forces of chemical affinity between two "doubly-linked" carbon atoms are not exerted in the imaginary straight line joining the atoms, but have to act, as it were, round a corner. Their effective value is weakened in accordance with the laws of the resolution of forces.

It has also been stated that the formula of Thomsen is impossible, because it represents a system of atoms which could not possibly be in equilibrium. This assertion involves the assumption that the forces which bind the atoms together act only in the directions *ab*, *bc*, *cd*, *de*, *ef*, *fa* (taken in order) and along *ad*, *bc*, and *cf*. It is in direct opposition to Newton's third law of motion. Each atom offers resistance to the interpenetration of its sphere of action by that of another atom. The force necessary to compress a liquid proves that this resistance exists in the case of molecules. This point may be illustrated by the fact that a model of Thomsen's symbol will hold rigidly together if made of six equal spheres, of which the centres are connected by flexible and inextensible strings *ab*, *bc*, *cd*, *de*, *ef*, *fa*, *ad*, *be*, and *cf* in such a manner that the strings *ab* to *fa* inclusive are each equal in length to the diameters of the spheres, whilst *ad*, *bc*, and *cf* are each equal to this length multiplied by $\sqrt{2}$. (See fig. 1.) The supposition that the spheres of action of the carbon atoms in benzene are so related is the only one consistent with the view that the atoms approach one another as nearly as possible. If the longer strings be cut it will be found possible to open out the model so that the centres of the spheres form the angular points of a plane hexagon. I do

not mean to imply by this model that the atoms are rigidly fixed, but that a given atom is unable to shift its mean position without altering the mean positions of each of the others.

The arguments by which I propose to distinguish between these octahedral formulæ and Kekulé's symbol may be thus classified:—

I. Evidence of direct linkage between symmetrically disposed carbon atoms (para-linkage) will be brought forward. Arguments derived from this evidence will support the symbols of Meyer and Thomsen as opposed to those of Kekulé and Armstrong.

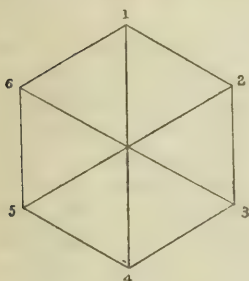
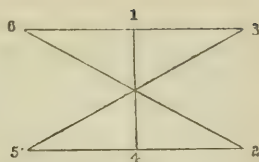
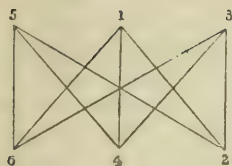
II. Arguments for Thomsen's as opposed to Meyer's formula will be based upon the constitutions of conine, of fluorene, and of the conjugated bodies, and upon the analogies of ortho- and para-compounds.

III. The symbol of Thomsen will then be further developed. This development will be supported by the crystallographic character of benzene, and will afford a rational explanation of the meta- and para- laws of substitution. A similar consideration of Meyer's symbol will fail to give this explanation.

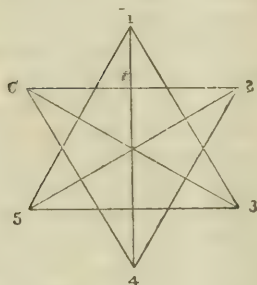
Arguments derived from the study of pyridine derivatives will be applied by analogy to the derivatives of benzene. Recent research entirely warrants such an assumption. Hartley found the selective absorption of the ultra-violet rays, characteristic of benzene and its derivatives, to be very strongly marked in the case of pyridine, picoline, quinoline, &c. (*J. C. S.* 1881, p. 153; 1882, p. 45). The recent paper by Horstmann, on the physical properties of benzene, fully bears out the analogy between the benzene and the pyridine nucleus (*Ber.* xxi. p. 2220, footnote). We shall see also that the independent consideration of benzene and pyridine derivatives leads to the same conclusion.

I should propose by the term "aromatic nucleus" to indicate an octahedral arrangement of six carbon or nitrogen atoms, characterized by a compactness of molecular structure which is due to the existence of para-linkage.

It will be sufficient in most cases to use one of the projections of each octahedral symbol, viz. the diagonal symbol of Claus and the star symbol of Ladenburg.



(Claus.)



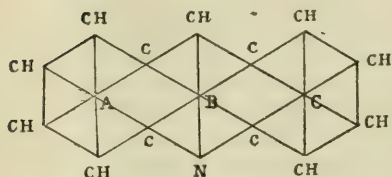
(Ladenburg.)

I. Arguments for Para-linkage.

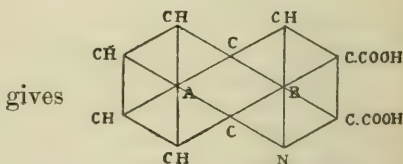
(1) Anthracene consists of three symmetrically conjugated aromatic nuclei. This view was formerly held, but had to be given up by the supporters of Kekulé's theory when Anschutz and Eltzbacher, in achieving the synthesis of anthracene, showed that the central carbon atoms are directly linked to one another (*Ber.* xvi. p. 623). It is now asserted that anthracene consists merely of two benzene rings united by a paraffinoid residue $(\text{CH}-\text{CH})^{\text{IV}}$. Several considerations show that the central nucleus is truly aromatic. In the first place Ramsay has shown that anthracene has, like benzene, naphthalene, and phenanthrene, an abnormally low molecular volume (*J. C. S.* 1881, p. 64). Hartley has also shown that the absorption of the ultra-violet rays observed in benzene is much increased in the case of anthracene as well as in those of naphthalene and phenanthrene. By means of oxidation the two central methenyl groups become severed and converted into carbonyl groupings. The body so produced is closely related to the quinones and its diketonic constitution has been well ascertained. The carbonyls can be reduced and the methenyls reunited. Such well-marked reactions are characteristic not of paraffinoid but of aromatic bodies. No instance is known in which a "paraffinoid" single

linkage can be broken and again set up in such a manner. The formation of anthracene from benzene and acetylene tetrabromide does not prove that the paraffinoid residue $(C_2H_2)^{IV}$ exists as such in the anthracene molecule, any more than Berthelot's synthesis of benzene from acetylene shows that there are three such residues in benzene. The aggregation of other atoms to this residue causes it to assume the most stable configuration, viz. that of two para-carbon atoms in the aromatic nucleus.

A direct proof of the aromatic nature of the central ring in anthracene is wanting. The sulphonic radical, when introduced by the direct action of sulphuric acid, invariably attaches itself to one of the external nuclei. The naphthalene derivatives, which we might expect to be the immediate products of the oxidation of anthracene, are at once further oxidized (Beilstein, *Handb. der org. Chemie*, ii. p. 188). It should be noticed that a proof of the contrary view would be no argument against para-linkage because di-phenylene ethane can be as well represented by Claus' as by Kekulé's symbol. It has, however, been shown (Graebe and Caro, *Ber.* xiii. p. 99) that acridine (the analogue of anthracene) is oxidized to a quinoline derivative, thus:—

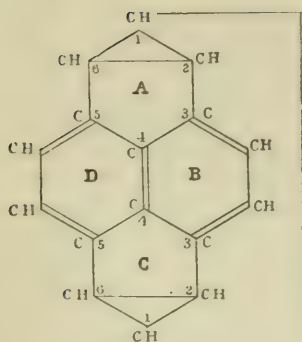


Acridine.

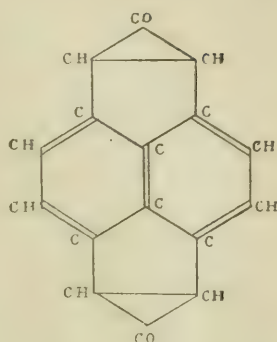
Pyr. α - β -quinoline dicarboxylic acid.

This reaction shows that the central ring (B) of the acridine molecule is a pyridine nucleus. By analogy it may be concluded that the central ring in anthracene is of a benzenoid nature. If this be admitted it follows, from the proved existence of a single linkage in this ring, that the benzene molecule must have one and therefore three such linkages.

(2) Bamberger and Philip have shown pyrene to consist of four benzene nuclei A, B, C, D, conjugated as in the diagram (*Ber.* xx. p. 365). These chemists give the following formulæ for pyrene and its quinone:—

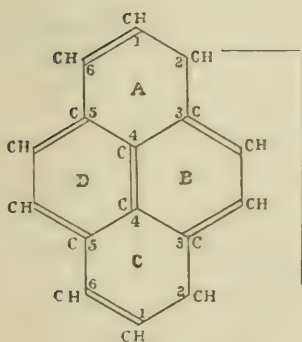


Pyrene, $C_{16}H_{10}$.

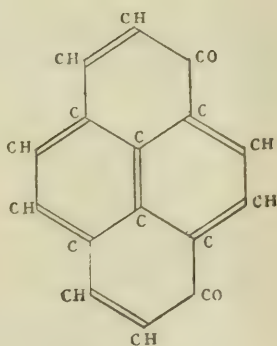


Pyrenequinone, $C_{16}H_8O_2$.

I.



Pyrene.



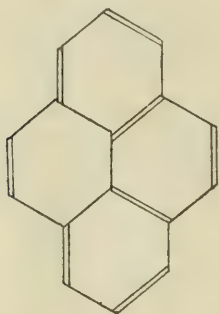
Pyrenequinone.

II.

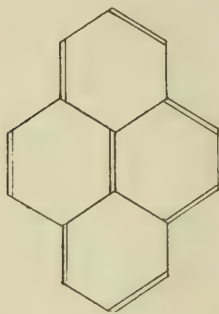
It is quite inconceivable that the carbon atom A1 should be directly linked to C1 as in formula I; or A2 to C2 as in II.

I shall endeavour to show that the proved constitution of this body is an important link in our argument. In the first place, it is quite impossible to represent it as a conjugation of four of Kekulé's rings.

Symbols such as

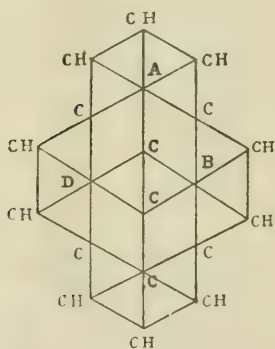


and

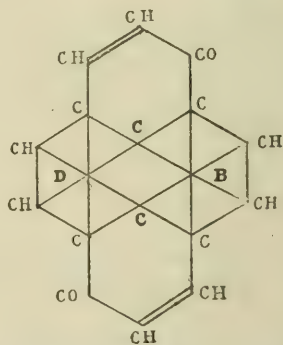


would lead to the constitution $C_{10}H_4 \left\{ \begin{array}{l} C_3HO_2 \\ C_3H_3 \end{array} \right.$ for pyrenequinone, whereas experiment shows that this body must be represented as $C_{10}H_4 \left\{ \begin{array}{l} C_3H_2O \\ C_3H_2O \end{array} \right.$ (See pyrenic acid &c., *Ber.* xx. p. 371.)

On the other hand, these bodies can be readily represented as conjugations of Thomsen's symbol (*vide infra* for adaptation of Meyer's symbol).



Pyrene, $C_{16}H_{10}$.
(Compare symbol on p. .)



Pyrenequinone, $C_{16}H_8O_2$ (a naphthalene deriv.). (*Vide infra*, constitution of benzoquinone.)

The facility with which they can be thus represented affords further evidence of para- as opposed to double-linkage.

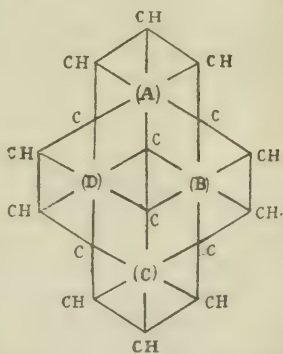
Here we may pause to consider the views of those chemists, who, in spite of recent research on the causes which determine isomerism, have refused to believe that in the symbol of Claus the para- are to be distinguished from the ortho-di-

derivatives. This objection was legitimate and perhaps necessary, so long as our chemical formulae were only convenient modes of representing atomic interactions. We are told that instead of para-linkages there are linkages directed from the carbon atoms towards the centre of the molecule, thus :—



Not a shadow of experimental evidence is brought forward to show that the valencies are of such a nature. It is difficult to conceive what function such valencies have. If valency means the direction along which the attractive force between two atoms can be exerted, rather than a vague notion of prongs sticking out from the atoms, it is hardly legitimate to suppose that a carbon atom can attract or be attracted by an empty point in space. We shall not discuss the relation of such a view to the "theory of open affinities," which is not only in conflict with the facts of isomerism generally, but was disproved by work on the isomers of propylene. It has been stated that these valencies, being directed towards the centre, are in a sense protected. If this is the case, and the para-carbon atoms are not directly combined, it will be found difficult to represent the constitution of pyrene, unless the fundamental basis of modern organic chemistry—the tetravalency of the carbon atom—be given up. The formula would consist of two distinct parts. These parts should exist as molecular entities (C_2 and $C_{14}H_{10}$), thus :—

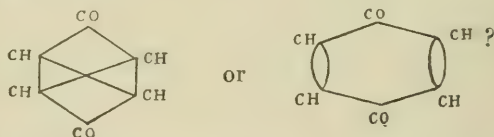
Such a supposition is not only unwarranted, but opposed to all that we know of pyrene. If the symbol merely represents, as is sometimes said, the idea that a given carbon atom is directly united with each of the other five, it is not easy to see how its supporters can explain the occurrence of more than one isomeric di-derivative, except by the consideration of the positions in space of the atoms. What advantage it would then have over that of Claus I will leave to be pointed out by those who propounded it.



(3) Thomsen states in his paper (*Ber.* xix. p. 2944) that the most stable bonds in benzene are those uniting para-carbon atoms, and that additive compounds are produced by the severance of one or more alternate peripheral bonds. A. K. Miller has shown that such a view is inconsistent with

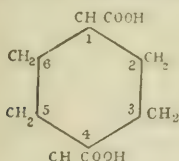
known facts (J.C. S. 1887, p. 214). Discredit has thus been thrown upon Thomsen's symbol for benzene. If, however, we suppose that the para-linkages are comparatively weak and that these are broken, the facts quoted by Miller cease to be inconsistent with the formulæ. Two carbon atoms may be bound together by three kinds of single linkage, which may be called ortho-, para-, and paraffinoid. By this I mean merely a difference in distance between the two atoms. Ortho-carbon atoms are closer together than two consecutive carbon atoms in a paraffinoid chain, and these than para-carbon atoms. The strength of chemical affinity varies inversely as a function of the distance between them. These points I hope to consider more fully in a future paper, and by defining them more precisely to afford a basis for the treatment of the physical properties of benzene. Meanwhile they afford some explanation of the stability of the aromatic nucleus, and of the formation of the additive compounds of benzene, pyridine, &c.

We may first take the quinones. The diketonic formula for benzoquinone seems to be placed beyond all doubt by Pechmann's synthesis of dimethyl-quinone from diacetyl (an undoubted carbonyl compound) (*Ber.* xxi. p. 1417). Only one benzoquinone is known, and this is a para-compound. From Kekulé's theory we should expect such a body to be ortho-. Its formation from and reduction to hydroquinone are best explained by the dissolution and re-establishment of a para-linkage (*vide supra*, anthraquinone, p. 408). But what becomes of the other two para-linkages in hydroquinone? Is the formula of quinone

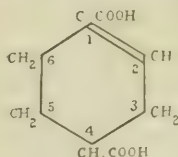


I am inclined to believe that when a para-linkage is broken the nucleus opens out into a hexagonal ring, and the remaining para-linkages are severed with the formation of true olefinoid linkages. The work of Baeyer on the additive compounds of terephthalic acid lends some support to this view (*Annal.* cexlv. pp. 103-185). He has described a series of four compounds—terephthalic acid and its di-, tetra-, and hexahydro compounds. It is not impossible to explain his results by the successive setting up of para-linkages in the passage from hexahydroterephthalic acid to terephthalic acid. But a curious fact, and one which seems to indicate the existence of

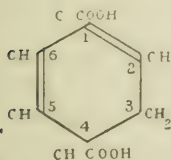
olefinoid linkages in these bodies, is that the two intermediate substances are much more readily disintegrated by oxidizing agents than the others. I may quote Baeyer's formulæ:—



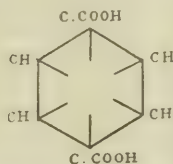
Hexahydro-acid (stable).



Tetrahydro-acid (unstable).



$\Delta^{1.5}$ dihydro-acid (unstable).



Terephthalic acid (stable).

The behaviour of the intermediate bodies towards bromine and hydrobromic acid, compared with that of terephthalic acid itself, shows that they are (in a different sense) unsaturated bodies.

We should rather expect a gradational change of properties, if one, two, and three para-linkages were successively formed. Whatever may be the view entertained of this question, it is nevertheless true that the results of Baeyer's work are inconsistent with Kekulé's symbol for benzene. Baeyer states that the obvious conclusion, from the reduction in one stage of the dibromide of the $\Delta^{1.5}$ dihydro-acid to terephthalic acid—viz. that terephthalic contains para- or meta-linkages—would involve him in serious inconsistency. It seems to me that in this respect he is illogical. Experiment justifies the belief that double, and not para-linkages, are set up in the intermediate compounds; but we are not thereby warranted in the assumption that para-linkages are not formed in the end reaction, which is admittedly of a different character. (In the preceding year he stated that benzene has a double-bond, because tetrahydro-terephthalic acid was thought to have one, *Ber.* xix. p. 1797.) The ultimate formation of the aromatic nucleus is brought about by a comparatively complicated change. Three para-linkages are simultaneously set up, and the atoms are drawn more closely together by the resulting pull towards the centre of the molecule.

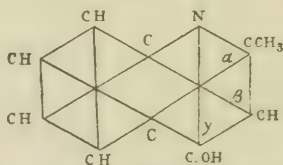
The symbol which he proposed, and which I have already discussed, has been somewhat improved by Marsh (*Phil. Mag.*

Nov. 1888). This chemist has derived it from six regular tetrahedra. It appears to me that Marsh's representation is inconsistent with the stability of the benzene nucleus, because the ortho-linkages closely resemble those characteristic of olefinoid compounds, which are admittedly a source of molecular weakness. Each consists of two valency-channels meeting at an angle (*vide supra*, p. 404).

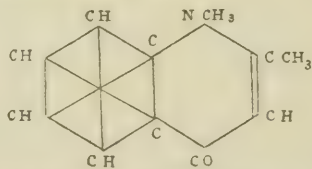
The researches of Nietzki (*Ber.* xviii. p. 504; xx. p. 322) on the secondary and tertiary quinones, and those of Meldola and Streatfeild (*Phil. Mag.* 1887, xxiii. p. 513; *J. C. S.* 1887, pp. 115 & 448), afford further arguments for the existence of para-linkage in benzene.

(4) Hantzsch (*Ber.* xvii. p. 1512) investigated the condensation of aceto-acetic ether with the aldehyde ammonias, and showed that in the pyridine derivatives so produced the γ -carbon atom is identical with that directly attached to the nitrogen of the aldehyde ammonia.

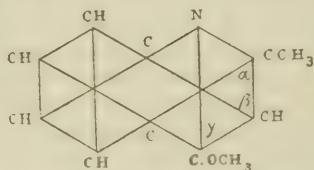
Knorr and Antrick (*Ber.* xvii. p. 2870) have shown that γ -oxyquinaldine (a quinoline derivative) is obtained by the action of aniline on aceto-acetic ether. This body they proved to be a "lactim" of the constitution



By direct alkylation it is converted into the "lactam" form of methyl- γ -oxyquinaldine, viz:—

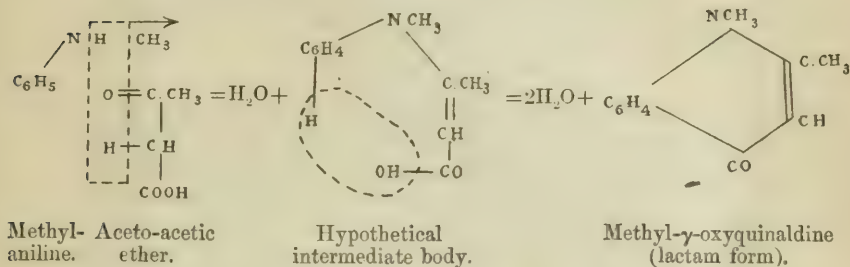


That this body is not the "lactim" form of γ -methoxyquinaldine, viz.



is shown by the fact that they prepared it by the action of

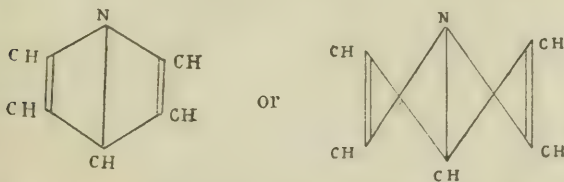
aceto-acetic ether on methyl-aniline, a reaction which can be represented only as follows :—



The formation of this body from γ -oxyquinaldine necessitates the change of the "lactim" into the "lactam form." This change can be satisfactorily explained only on the assumption that in γ -oxyquinaldine the nitrogen atom is directly combined with the para- or γ -carbon atom.

These researches, together with those of Ruhemann on citrazinic acid (*J. C. S.* 1887, p. 403), and of Graebe and Caro on acridine (*Ber.* xiii. p. 99), afford evidence of para-linkage in the pyridine nucleus.

Finally, I would point out that arguments in favour of one para-linkage in benzene are equally strong in favour of three, for in no other way can the necessary symmetry of the molecule be maintained. Such a deduction would not be strictly logical in the case of the pyridine compounds, but the only alternative formula—viz. one with olefinoid linkages,



is scarcely a representation of the stability of pyridine.

[To be continued.]

XLVIII. *On the Application of the Clark Cell to the Construction of a Standard Galvanometer.* By Professor RICHARD THRELFALL, M.A.*

[Plate XIV. figs. 1 & 2.]

IN the instrument which forms the subject of this paper the experience obtained with Clark's cell is utilized to obtain a simple means of standardizing a working current-measurer. The construction of the instrument itself will be readily understood from the accompanying figures. Its chief features are:—

(1) The arrangements which have been made for the support of the controlling magnet and for its adjustment: this latter can be readily carried out without disturbing the suspension.

(2) The damping of the needle by means of a thin copper cylinder attached to a bit of fine wire and dipping in clove-oil.

(3) The mechanical arrangements of this part of the apparatus, allowing of the easy suspending of the mirror and adjustment of the cylinder in the oil.

(4) The arrangements for the testing of the galvanometer by means of the Clark cell.

(5) The curving of the scale, so as to obtain direct tangent-readings from a scale of equal parts.

The single coil of the instrument consisted of a rectangular section of winding of 200 turns of No. 22 B.W.G. copper wire. Resistance 2·02 ohms at 16° C.

This coil had an axial dimension of 1·3 centim., a radial dimension of 1·3 centim., and the radius of the inner layer of winding was 3·7 centim. The coil was supported so as to be capable of sliding backwards and forwards with respect to the suspended parts, keeping parallel with itself to a considerable degree of accuracy. The slide was of carefully crossed wood, the moving portion being kept in its position in the grooves by means of half carriage-springs at each end. A somewhat similar arrangement is adopted in the sliding wooden parts of the Kew magnetometer.

There are three marked positions of the coil with respect to the suspended needles.

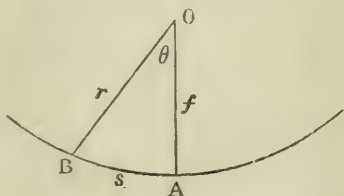
Though the mirror is rather large the magnets are small, in the ordinary sense, *i. e.* about a quarter of an inch long. A calculation was made by Mr. Adair on the law of deflexion of the magnet by the coil at the three distances and up to deflexions of about 15°. The method adopted in this calcula-

* Communicated by the Physical Society: read March 23, 1889.

tion was the expansion in 'Spherical Harmonics' used by Maxwell, part 4, chap. xiv. The tangent law was found to be practically true, *i. e.* the deviation from it would never introduce an error of more than about .5 per cent. The error was greater the less the deflexion, and was negligible for the accuracy required, which was of course not very great. The error arising from partly neglecting the torsion of the silk fibre was also investigated and found to be without influence: the fibre was seven inches long.

The divided scale was one of Elliott's scales, in which 360 divisions correspond to 229 millimetres. The distance from the mirror to the scale was 1095.7 scale-divisions, or about seven hundred millimetres. The problem of finding the form of the curve into which it is necessary to bend the scale of equal parts so as to read direct currents was solved by Mr. Adair; as we could find no previous record of this solution, I will give it here.

Let AB be a portion of the curve required; let $OA = f$ the apsidal distance, θ the polar angle subtended by AB , $OB = r$ the distance of the light-spot from the mirror. The incident light falls along AO . $AB = s$.



The form of the curve, assuming that the galvanometer obeys the tangent law, is

$$s = 2f \tan \frac{\theta}{2},$$

or

$$\frac{ds}{d\theta} = f \sec^2 \frac{\theta}{2};$$

and the differential equation giving r in terms of θ is

$$\left(\frac{dr}{d\theta}\right)^2 + r^2 = f^2 \sec^4 \frac{\theta}{2}.$$

This is insoluble in general terms: but if the range of θ is small we can develop in powers of θ , and assume $r = f + \lambda$, where λ is the addition to the radius of the circle whose centre is O and radius f . Thus the differential equation for λ becomes, by retaining terms in θ^4 ,

$$\frac{1}{f^2} \left(\frac{d\lambda}{d\theta}\right)^2 = \left[2\left(\frac{\theta}{2}\right)^2 + \frac{7}{3}\left(\frac{\theta}{2}\right)^4\right] - 2\frac{\lambda}{f}.$$

Writing λ' for $\frac{\lambda}{f}$, we get

$$\frac{d\lambda'}{d\theta} + \left[\frac{\sqrt{2}}{\theta} - \frac{7\sqrt{2}}{48} \theta \right] \lambda' = \frac{1}{\sqrt{2}} \left[\theta + \frac{7}{48} \theta^3 \right];$$

hence

$$\lambda' = \theta^{-\sqrt{2}} e^{\frac{7\sqrt{2}}{96} \theta^2} \left[c + \int \theta^{\sqrt{2}} \left\{ 1 - \frac{7\sqrt{2}}{96} \theta^2 \right\} \frac{1}{\sqrt{2}} \left\{ \theta + \frac{7}{48} \theta^3 \right\} d\theta \right].$$

Now $\lambda' = 0$ when $\theta = 0$; therefore the arbitrary constant $c = 0$; and to the order of θ retained we get

$$\begin{aligned} \lambda' &= \frac{\sqrt{2}-1}{2} \theta^2 + \frac{4-\sqrt{2}}{96} \theta^4 \\ &= .207 \theta^2 + .0269 \theta^4. \end{aligned}$$

At the extremity of the scale θ is about $\frac{1}{3}$ in circular measure; thus at the end of the scale,

$$\lambda' = .0233,$$

and $\lambda = 25.5$ scale-divisions.

Thus on the radius making the angle $\theta = 19^\circ 6'$, whose circular measure is $\frac{1}{3}$, the theoretical curve is outside the circle by a distance of 16 millim. Similarly, when $\theta = 10^\circ = .174$ radius, $\lambda' = .006297$, and therefore $\lambda = 6.901$ scale-divisions = 4.4 millim.

With these numerical results the curve was laid out on millimetre paper, a template was cut to the curve, and the wooden back of the scale-holder was brought up to the template. The scale itself was carefully pinned to the wood at short intervals along its whole length.

The Clark cell supplied with the instrument had the same area of surface as the "large cell" referred to in our previous papers: it was tested from day to day alongside of the large cell, master cell, &c. The following particulars refer to this testing.

Internal resistance (August 28, 1888), roughly 5.5 ohms. August 29, at 16° C., hospital cell — master cell = -0.000852 volt. Terminal E.M.F. hospital cell, two minutes after short-circuiting through 1426 legal ohms, taking E.M.F. of master cell at 1.435:—

August 28.	1.4263	volt.
„ 29.	1.4261	„
„ 29.	1.4262	„

About ten minutes after short-circuiting, in each case the E.M.F. fell further .0011 volt.

As the galvanometer was very dead-beat no calibration-experiment need take longer than 20 seconds, so that this is without effect.

In order to use the instrument the cell is coupled up in series with a platinoid resistance of 1417 legal ohms; the galvanometer-coil being itself 2·02 ohms, the cell about 5·5 ohms. Consequently the current used in testing is

$$c = \frac{1\cdot4262}{1417 + 2\cdot02} = \cdot001007 \text{ ampere.}$$

This is quite near enough for our purpose.

In order to set up the galvanometer once for all the following dispositions were made. The galvanometer-coil was pushed up towards the suspended magnet as far as it would go; this was known as position O. The controlling magnets were then raised so as to give a mean sensitiveness; the known current was put on and reversed and the double deflexion noted. This deflexion was indicated by a fiducial mark on the scale. If at any time the sensitiveness of the instrument changes, it is only necessary to bring the coil up to its O position and raise or lower the controlling magnets by means of the adjusting arrangement till the deflexion reaches the fiducial mark.

When the coil is in the position A, ·001 ampere corresponds to 10 scale-divisions; and at B, to 1 scale-division. These points were found by using a very large storage cell, whose E.M.F. was tested whilst the calibration was going on, and employing suitable resistances from a thick wire box. The whole arrangement was tested by this means from ·001 to ·4 ampere, and it was found that the results were wonderfully consistent: this was possibly in part due to the mirror not being very good, and consequently the observation is so far wanting in accuracy. With the rather bad light-spot the readings could not be taken nearer than to about 1 per cent. at the end of the range, and 3 per cent. towards the centre. Of course if currents of only three or four milli-amperes are to be measured, these can be got with at least this accuracy by using the coil at some convenient point near O, A and B being rather far away. As an accuracy to about 5 per cent. is all that is requisite in measuring currents for hospital work, there can be no doubt that this instrument fulfils the purpose for which it was made, having an accuracy in use of at least five times this amount. I have to thank the assistants in my laboratory for the excellency of their workmanship.

XLIX. *An Improved Standard Clark Cell with Low Temperature-Coefficient.* By H. S. CARHART.*

THE best form of Clark cell hitherto made is that of Lord Rayleigh, described in the 'Philosophical Transactions' for 1885. The objections to this form are that the temperature-coefficient is not the same for all cells, as is shown in Lord Rayleigh's paper, and it is so high as to introduce a very troublesome and uncertain error because of the difficulty of ascertaining the exact temperature of the cell; secondly, it is not so constructed mechanically as to prevent the mercury from coming into contact with the zinc when the cell is subjected to violent jars in transportation; thirdly, a great chemical defect is the facility with which local action takes place between the zinc and the mercury salt. I might add that the mercurous sulphate, purchased by Lord Rayleigh, evidently contained considerable salt in the mercuric form, as is shown by its turning yellow on mixing with the zinc-sulphate solution.

All these difficulties I have, at least in large measure, overcome. Respecting the materials, the greatest care is required to secure and maintain cleanliness and purity in their preparation. The mercury must be distilled *in vacuo* after being cleaned by chemical means. The zinc sulphate should be free from iron as well as other impurities. The mercurous sulphate can be made almost or quite free from the mercuric form by using plenty of mercury; keeping the temperature down to the lowest point at which action will take place; and letting the mixture of salt, acid, and metallic mercury stand for some time. I have made in this way a salt that remains white, not only when the free acid is all washed out, but when mixed with the standard zinc-sulphate solution. Further, it remains white in the cell indefinitely if it is not exposed to a bright light.

Hitherto the importance of the local action going on in a Clark cell appears not to have been appreciated. It accounts for some of the differences in temperature-coefficient, and leads to some more serious results in some cells. The zinc replaces mercury when in contact with the mercury salt. This amalgamates the zinc, producing a slight change in the E.M.F.; and then the amalgam is liable to creep up to the top of the zinc, where it attacks the solder. The copper wire is thus sometimes loosened. The zinc sulphate follows up, and the cell may be thus short-circuited by the zinc and the copper wire. Upon taking down one cell, which was perhaps a year

* From an advance proof communicated by the Author.

old, I found that the zinc had been removed from the rod at the surface of the liquid and had been deposited again upon the rod at the surface of the mercury salt, in a solid frill round the zinc. The copper wire in this cell became entirely detached, partly because of the expansion upward of the marine glue, which brought a severe strain upon the wire.

The local action then increases the zinc sulphate in the cell at the expense of the mercury sulphate and amalgamates the zinc rod. I have become convinced by some experiments extending over several weeks that this substitution process goes on only when the zinc is in contact with the solid mercury salt. The mercurous sulphate is only slightly soluble in a saturated solution of zinc sulphate. I therefore prevent local action by keeping the zinc and the mercury salt out of contact. The same device operates to raise the E.M.F. about 0.4 per cent. The following Table exhibits the observed and calculated values of the E.M.F. of cells No. 17, 112, 113 in terms of No. 1 (old style) at 20° C. :—

No. 17.			No. 112.		
Temp. C.	Observed.	Calculated.	Temp. C.	Observed.	Calculated.
8.3	1.0108	1.0106	5.1	1.0124	1.0125
8.5	1.0103	1.0105	10.6	1.0106	1.0103
9.3	1.0104	1.0102	12.5	1.0098	1.0096
11.8	1.0093	1.0092	15.2	1.0087	1.0086
13.8	1.0084	1.0085	19.5	1.0069	1.0069
15.0	1.0080	1.0080	21.2	1.0062	1.0062
18.1	1.0069	1.0068	31.1	1.0024	1.0024
19.4	1.0064	1.0063	No. 113.		
19.9	1.0062	1.0061			
20.3	1.0060	1.0059	5.1	1.0124	1.0125
20.8	1.0054	1.0057	10.6	1.0106	1.0104
21.1	1.0057	1.0056	12.5	1.0098	1.0097
21.6	1.0054	1.0055	15.2	1.0088	1.0087
22.4	1.0050	1.0052	19.5	1.0070	1.0070
23.3	1.0048	1.0048	21.2	1.0062	1.0063
25.1	1.0044	1.0041	31.1	1.0025	1.0025
26.4	1.0035	1.0036			
30.2	1.0019	1.0022			
33.1	1.0014	1.0013			
39.1	0.9991	0.9989			
41.7	0.9980	0.9979			
50.4	0.9949	0.9947			
52.7	0.9939	0.9940			

Cell No. 1 was always very near 20° C., and the reduction to that temperature was made by means of Lord Rayleigh's reduction-coefficient, .00077 per degree C.

The equation for the E.M.F., derived from the observations on No. 17, is

$$E' = E[1 - \cdot 000387(t-15) + \cdot 0000005(t-15)^2].$$

The calculated values for the three cells were all obtained by this formula. The change for one degree C. is, then, the following linear function of the temperature:—

$$- \cdot 000386 + \cdot 000001(t-15).$$

The temperature-coefficient ranges from $\cdot 000361$ at 0° C. to $\cdot 000376$ at 25° C., and to $\cdot 000361$ at 40° C. At the highest observed temperature in the preceding Table it was only $\cdot 000348$. The curve of E.M.F. with temperatures as abscissæ is clearly concave upward, indicating a fall in the temperature-coefficient with rise of temperature. The change is, however, so small as to be quite negligible within the range of temperature to which a normal element is subjected in practice. Lord Rayleigh's cells show a change in the temperature-coefficient directly the reverse of the above; that is, the coefficient increases by a very appreciable quantity with rise of temperature. For his No. [36] the coefficient ranged from $\cdot 000556$ at 0° C. to $\cdot 00101$ at 25° C., if his equation holds true for the higher temperature.

In making comparisons of E.M.F. I have used Lord Rayleigh's method, slightly modified, by means of which a difference of one ten-thousandth part is observed directly and with the greatest ease. In fact a difference of half that amount is easily measured. A comparison of half a dozen cells can be made in as many minutes without difficulty.

As to polarization, these cells show none with external resistance greater than 30,000 ohms. At 30,000 ohms the polarization is just discernible; and with 10,000 ohms it amounts to only one ten-thousandth part in five minutes. This fall in E.M.F. is less than the accidental differences between different cells in general, and much smaller than the almost unavoidable errors due to ignorance of the real temperature of the cell. If the cell is not closed on less than 10,000 ohms resistance, and only for a few minutes, the polarization may be entirely neglected.

As indicating the uniformity attained, the following relative values of the E.M.F. of six cells, only four days old, may be given:—9048, 9049, 9049, 9048, 9046, 9043. The last one was still approaching the others when last observed. Six cells of later construction gave the following relative values when less than two days old:—9182, 9182, 9182·5, 9182, 9182, 9182·5. The two sets of numbers do not represent at all the relative values of one set as compared with the other.

It will be seen from the Table that Nos. 112 and 113 never differ by more than one part in ten thousand at the same temperature.

Physical Laboratory,
University of Michigan.

I. Notices respecting New Books.

Watts's Dictionary of Chemistry.

Watts's Dictionary of Chemistry, revised and entirely rewritten.
By M. M. PATTISON MUIR, M.A., and H. FOSTER MORLEY, M.A.
D.Sc. Vol. II. Longmans, Green, and Co.

THE second instalment of this invaluable work maintains the high character of the preceding volume. Commencing with Cheno-cholic Acid, the work concludes with an article on Indigo which terminates on page 700. The list of contributors contains the names of some authors who contributed to the first volume together with several new writers; the list is a sufficient guarantee that the editors have secured the collaboration of some of the highest authorities on the special subjects treated of. Among the longer articles are those by Dr. Schunck on Chlorophyll, by Dr. McGowan on Cholesterine, on Chromium by Mr. Muir, and on Cinchona bark by Mr. David Howard. The article on Chemical Classification by Mr. Muir occupies over 20 pages and is followed by a very useful bibliographical list. The same author contributes the articles on Cobalt and its compounds, on the Laws of Chemical Combination, and on the Combining Weights of the elements. The article on Combustion by Prof. Thorpe might with advantage have been extended; in its present form it is entirely historical. About 9 pages are devoted to the subject of Crystallization, the writer being Mr. H. Baker, and a long article (about 24 pages) on Cyanic Acids is from the pen of Dr. Senier. Mr. Muir devotes over two dozen pages to the Cyanides, and a short article on Relative Densities is contributed by Miss Ida Freund. An excellent article by Prof. Threlfall on Dissociation, which extends to 28 pages, must be noted as one of the special features of the present volume, and an equally valuable article on Chemical Equilibrium is from the pen of Prof. J. J. Thomson. The article on Equivalency is written by Prof. Ira Remsen, and that on Explosion is by Prof. Threlfall. Dr. S. Rideal contributes a somewhat sketchy article on Fermentation and Putrefaction, and the bibliographical list of works relating to this subject is not quite as extensive as could have been wished. Prof. Thorpe writes on Flame, Mr. Veley on Formic Acid, and Prof. Ira Remsen on Formulæ. The article on Geological Chemistry is by Mr. F. W. Rudler, and is sufficiently excellent to make us regret that more space could not have been devoted to this important and little studied branch of the science. Prof. Japp contributes the article on the Hydrazines and Hydrazones. A long article on

Hydrogen by Mr. Muir, and 8 pages on Indigo by Mr. A. G. Green conclude our list of the chief contributions to this volume. Although we are bound to admit that the spirit of active research is not so widely spread here as it is on the Continent, in scientific literature we certainly can hold our own. The present work is a production which reflects the highest credit upon the editors and their staff.

Bernthsen's Organic Chemistry.

A Text-Book of Organic Chemistry. By A. BERNTHSEN, Ph.D., formerly Professor of Chemistry in the University of Heidelberg. Translated by GEORGE MCGOWAN, Ph.D. Blackie and Son.

THE author of this excellent little volume of about 500 pages has long been familiar to working chemists in this country for his brilliant investigations in synthetical organic chemistry, and especially for his well-known researches into the colouring-matters of the Methylene Blue series. It will be instructive to British manufacturing chemists to learn that Prof. Bernthsen, after having held a Professorship in a German University, has now become Director of the Scientific Department of the world-famed "Badische Anilin und Soda-Fabrik" at Ludwigshafen on the Rhine. Such an intimate relationship between pure science and its applications as is revealed by the transference of a University Professor to the Directorship of a laboratory associated with a factory is the very best illustration we have had in modern times of the way in which industrial advancement is insured abroad. The book before us may be described as a condensed epitome of the present state of knowledge concerning organic chemistry—full, accurate, and abreast of the most recent discoveries. The original work has been revised and brought up to date by the author expressly for this English edition. The arrangement adopted is well calculated to impress upon the student a sound knowledge of the chief characters of the compounds of the various groups, and the author has throughout kept in view the educational value of the branch of science on which he writes by treating the subject as a logically connected whole unburdened by the mass of purely descriptive detail which so often repels the student of organic chemistry. The introductory portion consists of thirty-two pages containing sections on the usual general subjects, such as analysis, determination of formulæ, isomerism and polymerism, homology, radicals, classification, physical properties, and fractional distillation. The remainder of the volume forming the Special Part deals with the different groups, classified in the first place into the two great divisions of Methane Derivatives and Benzene Derivatives. This is certainly preferable to the usual designations of "Fatty" and "Aromatic." The Methane derivatives are treated of under fifteen groups, viz. hydrocarbons, haloid derivatives, monatomic alcohols, alcoholic derivatives, aldehydes and ketones, monobasic acids, acid derivatives, polyatomic alcohols, polyatomic monobasic acids, dibasic acids, tri- to hexabasic acids, cyanogen compounds, carbonic acid derivatives, carbohydrates,

and lastly transition compounds to the benzene series. Under the second class we have first of all an excellent summary of the differences between the two great classes, some sixteen pages being devoted to the general theory of the constitution of benzene and its derivatives. Then follow the groups of hydrocarbons, halo'd derivatives, nitro-derivatives, amido-derivatives, azo- and diazo-compounds, sulphonic acids, phenols, alcohols, aldehydes, and ketones, acids, indigo group, diphenyl group, diphenylmethane group, triphenylmethane group, dibenzyl group, naphthalene, anthracene, and phenanthrene, pyridine and quinoline groups, terpenes and camphors, resins, glucosides, albuminous substances, &c.

The translator has done his part of the work well, although we detect distinct Teutonisms here and there. The proofs have had the advantage of being revised by the author. We can confidently recommend the work to both teachers and students, and we hope that in a future edition the translator will have an opportunity of modifying the nomenclature in certain cases so as to bring it more into harmony with that adopted in this country.

An Elementary Treatise of Mechanics, for the use of Schools and Students in Universities. By the Rev. ISAAC WARREN, M.A. (London: Longmans, 1889. Pp. 144.)

It may be in the recollection of some of our readers that in our issue for January 1887 the Rev. T. K. Abbott raised the question, "To what order of Lever does the Oar belong?" and proposed to show that "the vulgar conception of the oar as a lever of the first order is correct." Our author, in a note, after stating that the oar is commonly regarded by writers on Mechanics as a lever of the second kind, proposes to reconcile these apparently conflicting statements. We use the summary he himself gives of the results he arrives at, viz.:—(1) The oar must be regarded as a lever of the *second order* if the resistance acting at the rowlock be understood to include, not only the *external resistance* to the boat's motion, due to the action of the fluid in which the boat floats, but also the *reaction engendered by the person of the oarsman* when he pulls the oar. (2) If we consider *only* the resistance offered by the fluid to the boat's motion, it will be found that *this resistance* is related to the effort employed by the rower at the handle of the oar *in exactly the same way as if this resistance acted at the blade of the oar*, and as if the rowlock were the fulcrum, i. e. *practically as if the oar were a lever of the first order*. The author's work will be found on pages 129, 130, and he concludes thus:—"Whether this result might have been *à priori* predicted from the circumstance that the rowlock is a fixed point relatively to the rower, the author leaves for others to determine." The text forms a handy book for junior students, and is accompanied by full store of illustrative exercises, with several specimen (Trinity College, Dublin) papers.

It should be mentioned that the present is the first part of the complete treatise, and is concerned (in the text) with Statics only.

Phil. Mag. S. 5. Vol. 28. No. 174. Nov. 1889. 2 I

LI. *Intelligence and Miscellaneous Articles.*

ON A RELATION BETWEEN THE SUN-SPOT PERIOD AND THE PLANETARY ELEMENTS. BY CHARLES DAVISON, M.A., MATHEMATICAL MASTER AT KING EDWARD'S HIGH SCHOOL, BIRMINGHAM.

THE length of the sun-spot period was first estimated by its discoverer, Schwabe, at about ten years. Some years later, Rudolf Wolf, making use of a much more extensive series of observations, determined the mean period to be 11·111 years, with an uncertainty of 0·307 year. The period of Jupiter being 11·86 years, it was at first surmised that there might be some connexion between the two. But the idea was soon abandoned, partly on account of the obviously considerable difference between the two periods.

A close approximation to the sun-spot period is, however, obtained by taking the average of the periods of all the known planets in the solar system, on the supposition that the determining effect of each planet is directly proportional to its mass and inversely proportional to the square of its distance from the sun. If m be the mass of a planet, d its distance from the sun, P its period, the average to be determined is

$$\Sigma(Pm/d^2) \div \Sigma(m/d^2).$$

In the following Table the values of m , d , and P are taken from Herschel's 'Outlines of Astronomy' (1873); the corresponding elements for the satellites and minor planets being omitted as unknown or unimportant.

Planet.	Mass.	Distance.	Period, in days.	m/d^2 .	Pm/d^2 .
Mercury	0·074	0·387	88	0·494	43
Venus	0·895	0·723	225	1·711	386
Earth	1·000	1·000	365	1·000	365
Mars	0·134	1·524	687	0·058	40
Jupiter	343·125	5·203	4333	12·676	54925
Saturn	102·682	9·539	10759	1·128	12136
Uranus	17·565	19·182	30687	0·048	1473
Neptune	19·145	30·057	60187	0·021	1264

From these values we find that

$$\Sigma(m/d^2) = 17·136,$$

$$\Sigma(Pm/d^2) = 70632,$$

and

$$\Sigma(Pm/d^2) \div \Sigma(m/d^2) = 4122 \text{ days, or } 11·29 \text{ years.}$$

If the elements be those given in Newcomb's 'Popular Astronomy' (1878), the value of this average is 11·27 years. The effect of taking the moon into account is to reduce both these estimates by 0·01 year. In either case the average is well within the limiting values given by Wolf for the sun-spot period, namely $11·111 \pm 0·307$ years.

ON THE LEAKAGE OF NEGATIVE ELECTRICITY CAUSED BY SUN AND DAYLIGHT. BY T. ELSTER AND H. GEITEL.

In reference to the theory of atmospheric electricity propounded by Arrhenius*, we have recently made a series of experiments to ascertain whether sun- and daylight have the property of gradually withdrawing the charge from negatively electrified bodies. M. Hoor† alone has established such an action, while all other observers, so far as we know, have not been able to discover any. We have therefore been greatly surprised to find that not only sun- but also ordinary diffused daylight can under suitable conditions rapidly discharge a negatively electrified body.

A zinc dish, 20 centim. in diameter, is exposed in the open on an insulating support in such a manner that it is not acted on by negative electricity, and is put in conducting communication with a quadrant-electrometer or an Exner's electroscope, and further is so arranged that the dish can be put in the dark or light at pleasure. The following phenomena can then be observed, which, it is true, are already known from experiments on ultra-violet light. The dry dish polished with emery completely loses a negative charge of 300 volts in 60 seconds: an equally high positive charge is retained. The loss of the negative charge ceases as soon as the dish is put in an entirely dark room; it is considerably enfeebled if the sun's rays pass previously through a glass plate. A decided collapse of the leaves of the electroscope takes place when the dish is merely exposed to the blue light of the sky.

If the dish is filled with hot or cold water the action is completely extinguished; a moist cloth stretched over it acts in like manner.

By being illuminated the finely polished plate acquired a spontaneous charge of +2.5 volts, which by blowing on the plate could be still further increased.

The experiments are much simpler when the metals to be illuminated are directly fixed in the form of wire to the knob of an Exner's electroscope. If freshly polished wires are used—aluminium, magnesium, or zinc—a permanent negative electrification in the sunlight in the open is not at all possible. It is completely discharged in less than five seconds. Magnesium and aluminium wires act here more energetically than zinc ones. There is a perceptible collapse of the leaves when the former are used, even with the action of diffused evening light.

It is also interesting to note that freshly polished wires of the metals in question act as if an ignited body were attached to the electroscope. If an electroscope so arranged is taken to an open field, the leaves diverge with the use of freshly polished wires with positive electricity, arising from the influence of the electricity of the air.

In all these cases an abnormal diffusion of positive electricity could not be observed.

* *Meteorol. Zeitschrift*, v. p. 297 (1888).

† *Rep. der Phys.* xxv. p. 105 (1889).

The experiments were made from the middle of May to the middle of June in this year (1889).—Wiedemann's *Annalen*, vol. xxxviii. p. 40 (1889).

ON THE PHOSPHORESCENCE OF COPPER, BISMUTH, AND MANGANESE IN THE SULPHIDES OF THE ALKALINE EARTHS. BY V. KLATT AND PHILIPP LENARD.

The results of a long series of researches on the phosphorescence of the alkaline earths are summed up by the authors as follows:—

(1) The strongly luminous lime-phosphorescents are mixtures of three essential constituents; sulphide of calcium, the active metal, and a third body which, when present alone in calcium sulphide, is not active. It is very probable that perfectly pure calcium sulphide does not phosphoresce.

(2) The bands which occur in the spectra of the lime-phosphorescents show that the active metals are manganese, copper, bismuth, and a fourth metal which is not known. To each of these metals a band corresponds which is invariable in position.

Extremely small quantities of the metal are active. The intensity of the phosphorescence at first increases with its quantity, and then decreases to zero. The quantities which exhibit the maximum action are very small.

(3) The additions used by us as the third constituent are colourless salts, and all of them fusible at the temperatures at which the phosphorescents are prepared. Hence they coat the surface of the calcium sulphide causing the mass to sinter together, and the active metal produces a delicate tint which is essential for the phosphorescence.—Wiedemann's *Annalen*, vol. xxxviii. p. 90 (1889).

ON STEATITE AS A SOURCE OF ELECTRICITY.

BY M. MENTZNER.

Steatite, when rubbed with gun-cotton, or with Kienmayer's amalgam, or with fur, becomes negatively electrified, and in the electrical series is on the extreme left. For these experiments a prism with rounded edges, 8 centim. in length, 3.5 centim. in breadth, and 3 centim. in thickness is used, in which there is a hole for an ebonite rod. In the upper surface a semicylindrical groove is cut by means of a glass rod and emery-paper. If a glass or ebonite rod is drawn through it, it becomes positively and the steatite negatively electrical.

A kind of electrical machine may be made from a cylindrical rod of steatite fixed to a handle of ebonite; a loop of copper wire is fastened round it, the ends of which are twisted together and terminate in a knob, which is held over a flat metal vessel full of bisulphide of carbon. When the steatite is struck with a fox's brush the bisulphide is ignited by the spark produced.—*Zeitschrift für phys. und chem. Unterricht*, ii. pp. 241–243 (1889); *Beiblätter der Physik*, vol. xxii. p. 707 (1889).

THE
LONDON, EDINBURGH, AND DUBLIN
PHILOSOPHICAL MAGAZINE
AND
JOURNAL OF SCIENCE.

[FIFTH SERIES.]

DECEMBER 1889.

LII. *On the Law of Cooling, and its bearing on certain Equations in the Analytical Theory of Heat.* By CHARLES H. LEES, *M.Sc., Berkeley Fellow of Owens College, Manchester**.

ALTHOUGH it is a well-known fact that the temperature of a heated body allowed to cool in air does not follow "Newton's Law," it has been usual to assume that law to hold in cases in which the loss of heat of a body through contact of its surface with air had to be taken into account. In calorimetry the error thus introduced is probably small; but it becomes of much greater importance in those methods of determining thermal conductivity in which the ratio of the outer to the inner conductivity of the body is a quantity determined experimentally, and this ratio used in conjunction with a value of the outer conductivity † (supposed to follow Newton's law) to determine the inner conductivity.

The method to which I specially refer is that of Briot, Despretz, &c., in which a bar of the substance whose conductivity is required is placed in a horizontal position in air, and heated at one end. The equation of motion of heat in the bar is then, assuming the isothermal surfaces to be planes,

$$cp \frac{\partial v}{\partial t} = \frac{\partial}{\partial x} \left(k \frac{\partial v}{\partial x} \right) - \frac{p}{q} . hv; \quad (1) \ddagger$$

* Communicated by the Author.

† The "*Conductibilité extérieure*" of Fourier, or the surface emissivity of Thomson, art. "Heat," *Encyc. Brit.* p. 577.

‡ Fourier, *Théorie Analytique de la Chaleur*. Thomson, 'Collected Papers,' vol. ii. p. 42, or *Encyc. Brit.*, art. "Heat," p. 579.

where v = excess of temperature at a point x of bar above temperature of air, which is supposed constant; c = specific heat, ρ = density, k = inner conductivity, h = outer conductivity, at temperature-excess v ; p = perimeter of section of bar; q = area of section of bar.

Assuming, with Fourier, that c , ρ , k , h are constant, we have for the "steady state" the equation

$$\frac{\partial^2 v}{\partial x^2} = \frac{ph}{qk} \cdot v;$$

the solution of which is

$$v = Ae^{\sqrt{\frac{ph}{qk}} \cdot x} + Be^{-\sqrt{\frac{ph}{qk}} \cdot x},$$

where A and B are constants of integration. By determining the temperature at different points of a bar thus heated, the

value of $\sqrt{\frac{ph}{qk}}$ can be found. Briot and Despretz determined

the temperature by means of thermometers placed in holes in the bars. This would, on account of the different conductivities of the bars and the glass and mercury of the thermometers, make the isothermal surfaces deviate considerably from planes. The results, however, of both experimenters show

evidence of an increase of $\sqrt{\frac{ph}{qk}}$ with temperature, especially

where the bars are of the poorer conducting metals, and the effects of the holes therefore less. This is also the case with the results of Wiedemann and Franz, who determined the temperature by means of a thermojunction brought into contact with the surface of the bar at different points.

From the experiments which follow it will be seen that h increases about 50 per cent. as the temperature varies from 30° C. to 80° C., while k and c for a metal are not found to vary more than about 5 per cent. in the same interval. The most important source of error in the assumptions of Fourier is, then, that introduced by the variation of h , and it is towards a more accurate determination of this variation that the following experiments have been conducted*.

A bar of infinite length originally heated to a uniform temperature would, if allowed to cool in air, continue to satisfy the condition $\frac{\partial v}{\partial x} = 0$, and it will be assumed that for the bars used in these experiments this is still the case. Since the

* The experiments were carried out in the Owens College Physical Laboratory.

temperature has previously been assumed constant throughout any section perpendicular to the axis, this amounts to assuming the temperature uniform throughout the whole bar. The equation (1) then takes the form

$$cp \frac{\partial v}{\partial t} = -\frac{p}{q} \cdot h \cdot v.$$

Or, multiplying both sides of equation by l , the length of the bar, and writing more generally for v on right side $f(v)$,

$$cp \, lq \frac{\partial v}{\partial t} = -plh \cdot f(v).$$

Or, writing m for ρlq , and s for the whole surface of the bar, including the two ends,

$$cm \frac{\partial v}{\partial t} = -s \cdot h \cdot f(v). \quad . \quad . \quad . \quad . \quad . \quad (2)$$

The bars used in these experiments to determine the form of $f(v)$ were about 26 or 27 centim. long and 1.9 centim. diameter. They were nickel-plated, so as to give them the same smooth even surface. At the extremities of a diameter in the middle of the length of the bars, two small holes, about .7 millim. diameter and 2.5 millim. deep, were bored, and into them an iron and a German-silver wire respectively were soldered. The other ends of these wires dipped into mercury-cups, in the circuit of an aperiodic Wiedemann's galvanometer. The mercury-cups being provided with thermometers, the arrangement formed a thermo-circuit, and from the deflexion of the galvanometer and the temperature of the mercury-cups the temperature of the bar at any time could be found. The deflexion was read off by means of a telescope and scale situated 2.5 metres from the galvanometer. The correction of the extreme deflexion for the assumption $\tan 2\theta = 2 \tan \theta$ is less than .3 per cent. and is neglected. The bars were at first suspended horizontally in the room by means of two threads, and the temperature of the air observed by means of a thermometer situated vertically under the bar and protected from radiation by a small paper screen. On account of the air-currents in the room, and the "lag" of the thermometer-indications behind the actual temperatures of the air, the cooling was ultimately carried out in a water-jacket 55 centim. diameter and 75 centim. long. The change of temperature of the jacket was then slow and regular, and the correction for this change could easily be applied. The bars were heated in an air-bath surrounded by water at 100° C. for three or four hours, and at the end of that time the temperatures of

bath and mercury-cups, the deflexion, and the resistance of the circuit were determined. The resistance was ascertained by shunting into circuit a known resistance, and observing the diminution of deflexion. These observations give data for reducing the observations of deflexion and resistance during cooling to degrees Centigrade. During cooling observations of deflexion were taken every two minutes; of resistance, and of temperatures of water-jacket and mercury-cups, every four minutes.

Writing e for the product of the deflexion and resistance, v temperature of junction in bar, v_1 temperature of mercury-cups, \bar{e} , \bar{v} values of e , v when bar is in hot bath, C some constant, we have, according to Avenarius*, for an iron-German-silver circuit,

$$\begin{aligned} e &= C(v - v_1)(1 - 0.00034(v + v_1)), \\ \bar{e} &= C(\bar{v} - v_1)(1 - 0.00034(\bar{v} + v_1)); \\ \therefore v - v_1 &= (\bar{v} - v_1) \frac{e}{\bar{e}} \cdot \frac{1 - 0.00034(\bar{v} + v_1)}{1 - 0.00034(v + v_1)} \\ &= \frac{\bar{v} - v_1}{\bar{e}} \cdot e(1 - 0.00034(\bar{v} - v)) \text{ approx.} \end{aligned}$$

Writing for v , in correcting factor, its approximate value $\frac{e}{\bar{e}} \bar{v}$, we have

$$v - v_1 = \frac{\bar{v} - v_1}{\bar{e}} \cdot e \left(1 - 0.00034 \bar{v} \frac{\bar{e} - e}{\bar{e}} \right),$$

which determines $(v - v_1)$, in Centigrade degrees, from observation of deflexions and resistance.

By applying this equation to the observations made, we get a series of temperatures of the bar at two-minute intervals; and from this series the form which $f(v)$ must have to best represent the variation of the outer conductivity is to be found. We consider only simple forms of $f(v)$, so as to complicate equation (1) as little as possible, and commence with the simplest.

The simplest form is $f(v) \equiv v$, the usual assumption. Substituting in (2), we deduce

$$\int \frac{dv}{v} + at = C, \text{ where } a = \frac{sh}{cm} \text{ (C is an arbitrary constant),}$$

or $v = v_0 e^{-at}$, where v_0 is value of v at time $t = 0$.

If a in this equation be determined from the first eight

* Pogg. Ann. cxxii. p. 199.

minutes of the curve of cooling, when the mean temperature-excess of the bar was about 64° C., it is always 30 or 40 per cent. greater than a determined from the last eight minutes when the temperature-excess was 11° C. Hence a increases with the temperature, and we are led to the assumption:— $f(v) \equiv v(1 + bv)^*$, which gives

$$\int \frac{dv}{v(1 + bv)} + at = \text{constant},$$

or

$$\frac{1}{v} + b = Ae^{at}, \quad \text{where } A \text{ is an arbitrary constant.}$$

The constants of this equation, determined from observations at times 0, 10, 20 minutes, are in one experiment

$$a = \cdot 0277, \quad b = \cdot 0047, \quad A = \cdot 0187;$$

and at 40, 50, 60 minutes, in the same experiment,

$$a = \cdot 0221, \quad b = \cdot 0165, \quad A = \cdot 0284.$$

A similar variation was found in other cases, so that the cooling is not well represented by making $f(v)$ a quadratic function of v .

Ceasing to consider integral powers of v , we write

$$f(v) \equiv v(a + bv^m),$$

where m is some \pm quantity. This gives as integral,

$$v^{-m} + \frac{b}{a} = Ce^{amt}.$$

Solving this by trial we find $m = \cdot 2$ approx. and $a = 0$; and we deduce as probable form $f(v) \equiv v^n$, where $n > 1$.

The equation (2) therefore takes the form:—

$$cm \frac{\partial v}{\partial t} = -sh \cdot v^n; \quad . \quad . \quad . \quad . \quad (2')$$

or the rate of loss of heat from the bar varies as the n th power of the excess of temperature of bar above temperature of air, supposed to remain constant, where $n = 1\cdot 2$ approximately.

So far the specific heat c has been considered constant; but

* Kundt and Warburg (Pogg. Ann. clvi.) make use of this to express the cooling of a thermometer in a sphere concentric with its bulb. H. F. Weber (*Mon. Ber. d. Berlin Akad.* 1880) considers some correction of this form to be necessary in dealing with conductivities of bars.

the form of (2') allows its variation to be taken into account without materially affecting the integration. Writing

$$c = c_0(1 + c'v),$$

where c_0 is the value of c at the temperature of the air in the experiments (about 17° or 18° C.), and c' is some constant, generally less than $\cdot 001$, (2') becomes

$$mc_0(1 + c'v) \frac{\partial v}{\partial t} = -sh.v^n.$$

Another small correction has to be applied for the change of temperature of the water-jacket, which up to the present has been assumed constant. Writing it now $= V$, where V is a function of t such that its value at the end of the experiment $= 0$, we have

$$mc_0(1 + c'v) \frac{\partial v}{\partial t} = -sh(v - V)^n.$$

From the Tables which follow, it will be seen that V/v is generally less than $\frac{1}{100}$; so that if, in the left-hand side of the above equation, $1 + c'(v - V)$ be substituted for $1 + c'v$, the error introduced is generally less than $\frac{1}{10000}$. Also from these Tables it is seen that, for an interval of twenty minutes, $\frac{\partial V}{\partial t}$ is with close approximation $= -q \frac{\partial v}{\partial t}$, where q is generally less than $\frac{1}{100}$. Hence, for an interval of twenty minutes, we have as a very close approximation,

$$\frac{\partial v}{\partial t} - \frac{\partial V}{\partial t} = \frac{1}{1+q} \frac{\partial v}{\partial t},$$

or

$$\frac{\partial v}{\partial t} = \frac{1}{1+q} \cdot \frac{\partial v - V}{\partial t};$$

and the equation of cooling corrected for all known variations becomes

$$\frac{mc_0(1 + c'v - V)}{1+q} \cdot \frac{\partial v - V}{\partial t} = -sh(v - V)^n;$$

or

$$\int \{ (v - V)^{-n} + c'(v - V)^{1-n} \} dv + \frac{sh(1+q)}{mc_0} t = \text{constant};$$

or

$$(v - V)^{-n-1} \left(1 - \frac{n-1}{2-n} c'(v - V) \right) = \frac{sh(1+q)(n-1)}{mc_0} t + C. \quad (3)$$

Tables follow from which it can be seen how this equation agrees with experiment.

Nickel-plated Copper bar cooled in water-jacket.

t , minutes.	$V^{\circ}\text{C.}$	$(v-V)^{\circ}\text{C.}$	$(v-V)^{-21} \left(1 - \frac{n-1}{2} e^{v-V} \right)$	$20 \frac{sh}{m_0} \frac{1+q}{n-1}$	$20 \frac{sh}{m_0} \frac{n-1}{n}$	$\frac{sh}{m_0} \frac{n-1}{n}$	
0	16.97	73.4	.4040				
2	17.05	68.1	102			Means	
4		63.3	168			of 5.	
6	.15	58.8	236				
8		54.62	299				
10	.21	50.83	366				
12		47.35	434				
14	.27	44.19	499				Another
16		41.28	567				exp. gave
18	.31	38.54	633				
20		36.06	700	.0660	.0653		
22	.35	33.67	770	68	61		
24		31.60	835	67	60		
26	.36	29.71	896	60	54		
28		28.00	958	59	53	.003281	.003264
30	.38	26.26	.5028	62	57		
32		24.61	096	62	57		
34	.39	23.06	167	68	63		
36		21.82	227	60	56		
38	.39	20.60	292	59	55	3288	3232
40		19.39	358	58	54		
42		18.40	425	55	52		
44		17.34	489	54	52		
46	.39	16.39	550	54	52		
48		15.50	621	53	52	3262	3209
50		14.65	685	57	56		
52		13.90	751	55	54		
54	.39	13.15	815	48	48		
56		12.47	883	56	56		
58		11.88	943	51	51	3265	3247
60		11.26	.6008	50	49		
62	.40	10.69	079	54	53		
64		10.16	141	52	51		
66		9.67	206	56	55		
68		9.19	273	52	51	3259	3272
70	.40	8.76	337	52	51		
72		8.36	399	48	47		
74		7.95	467	52	50		
76		7.58	531	48	46		
78	.42	7.26	592	49	47	3241	3220
80		6.93	654	46	44		
Mean... .003266							.003240

$$n = 1.21.$$

$$c_0 = .092.$$

$$\therefore h = .00325 \times \frac{1}{n} \times \frac{676}{\dots}$$

Nickel-plated Iron bar cooled in water-jacket.

t , minutes.	V° C.	$(v-V)^\circ \text{C.}$	$(v-V)^{-21} \left(1 - \frac{n-1}{2-n} e^{\frac{v-V}{c_0}}\right)$	$\frac{20^{sh} \frac{1+q}{n} \frac{n-1}{c_0}}$	$\frac{20^{sh} \frac{n-1}{c_0}}$	$\frac{sh \frac{n-1}{c_0}}$	
0	18.36	74.77	.3965			Means	
2	.44	69.78	.4031			of 5.	
4		65.10	.094				
6	.57	60.72	.159				
8		56.74	.223				
10	.67	53.04	.291				
12		49.73	.348				
14	.72	46.92	.404				
16		43.70	.474				
18	.78	40.92	.540				Another
20		38.42	.603	.0638	.0631		exp. gave
22	.81	36.02	.669	.38	.31		
24		33.91	.731	.37	.30		
26	.85	31.88	.795	.36	.29		
28		30.00	.858	.35	.28	.003149	.003198
30	.87	28.28	.920	.29	.23		
32		26.67	.983	.35	.30		
34	.89	25.09	.5049	.45	.40		
36		23.78	.108	.34	.29		
38	.93	22.55	.169	.29	.24	.3136	.3187
40		21.25	.236	.33	.28		
42	.96	20.00	.305	.36	.31		
44		19.02	.363	.32	.27		
46	.98	17.91	.431	.36	.30		
48		16.96	.495	.37	.30	.3146	.3161
50	.99	16.16	.552	.32	.25		
52		15.27	.621	.38	.31		
54	19.01	14.51	.682	.33	.26		
56		13.79	.745	.37	.30		
58	.03	13.06	.815	.36	.29	.3141	.3147
60		12.41	.875	.39	.32		
62	.03	11.84	.934	.29	.22		
64		11.25	.3000	.37	.31		
66	.03	10.71	.062	.31	.26		
68		10.20	.127	.32	.28	.3139	.3180
70	.03	9.79	.179	.27	.23		
Mean... .003139							.003179

$n = 1.21.$
 $m = 573.4 \text{ grams.}$
 $s = 162.2 \text{ sq. centims.}$

$c_0 = .112.$
 $c' = .00101.$

$$\therefore \frac{h}{c_0} = \frac{.00316}{.21} \times \frac{573.4}{162.2} = .0520.$$

Nickel-plated Zinc bar * cooled in water-jacket.

t , minutes.	V° C.	(v-V)° C.	$(v-V)^{-21} \left(1 - \frac{n-1}{2-n} e^{v-V} \right)$.	$\frac{20}{m c_0} \frac{sh}{1+q} \frac{n-1}{n-1}$.	$\frac{20}{m c_0} \frac{sh}{n-1}$.	$\frac{sh}{m c_0} \frac{n-1}{n-1}$.	
0	17.00	70.93	.4050			Means of 5.	
2	.10	64.92	122				
4		59.02	215				
6	.20	54.01	297				
8		49.33	381				
10	.24	45.10	466				
12		41.29	553				
14	.27	37.98	637				
16		35.00	717				
18	.29	32.33	798				
20		29.95	877	.0827	.0820		Another exp. gave
22	.29	27.70	964	42	37		
24		25.59	.5045	30	25		
26	.29	23.69	126	29	24		
28		21.99	211	30	26	.004132	.004100
30		20.30	301	35	33		
32		18.98	374	21	19		
34	.29	17.77	447	10	08		
36		16.60	531	14	12		
38		15.42	614	16	15	4087	4087
40		14.43	693	16	16		
42	.29	13.50	777	13	13		
44		12.62	862	17	17		
46		11.83	946	20	20		
48		11.05	.6031	20	20	4086	4109
50	.29	10.41	108	07	07		
52		9.79	185	11	11		
54		9.22	268	21	21		
56		8.62	353	22	22		
58		8.08	440	26	26		
60	.29	7.64	517	24	24	4093	4112
Mean...004100							.004102

$$\begin{array}{l}
 n = 1.21. \quad c_0 = .0915. \\
 m = 573.4 \text{ grams.} \quad c' = .000485. \\
 s = 165.5 \text{ sq. centims.}
 \end{array}
 \quad \left| \quad \therefore \frac{h}{c_0} = \frac{.00410}{.21} \times \frac{573.4}{165.5} = .0676.
 \right.$$

* Nickel surface not good, peeled in places.

The constancy of the numbers in the columns of first differences (the columns headed $20 \frac{sh \overline{n-1}}{mc_0}$) shows how well the assumption that the rate of loss of heat varies as the n th power of the temperature-excess can be made to represent the actual fact by a proper choice of n . Within the limits of the experiments, *i. e.* 80° to 10° C. temperature-excess, there is no definite secular change of the mean values of $\frac{sh \overline{n-1}}{mc_0}$, and we thence conclude that the above law is a close approximation to the actual fact.

The values of h/c_0 deduced from the experiments are, for copper, iron, and zinc, respectively $\cdot 0640$, $\cdot 0520$, $\cdot 0676$, where h is the amount of heat lost from 1 sq. centim. of the surface in 1 minute, when the temperature-excess is 1° C.* and temperature of air is about 18° C., and c_0 is the specific heat of the material of the bar at 18° C.

Taking the specific heats at 18° C. as $\cdot 092$, $\cdot 112$, $\cdot 0915$ respectively†, and dividing by 60, we have for the amount of heat lost in 1 second under the above conditions:—

$$\cdot 0000981, \cdot 0000971, \cdot 000103.$$

On account of the uncertainty of the specific-heat values, and the fact that the zinc bar had not so good a surface as the others, not much weight is to be attached to the differences between these three results. The value deduced from the copper bar is probably nearest the truth; and we have then the loss of heat in 1 second from 1 sq. centim. of surface of the bars used equal to $\cdot 000098(v-V)^{1\cdot 21}$ heat-units.

The particular value $1\cdot 21$ of the index n refers only to the cooling in the water-jacket, but it seems not to depend to any great extent on the presence or not of the jacket, for when the cooling was performed in the middle of the room at a considerable distance from any object the value of n which best represented the cooling was less than $1\cdot 21$ but greater than $1\cdot 2$.

The value of n is, however, dependent on the nature of the surface, and also on the cross section of the bar; for by covering the iron bar with a shining black varnish n was

* That is, if the loss of heat varies for temperatures below 10° C. excess, as it is found to do from 80° to 10° C. excess.

† Naccari gives:—

$$\left. \begin{array}{l} \text{Cu } \cdot 092 \\ \text{Fe } \cdot 106 \\ \text{Zn } \cdot 0915 \end{array} \right\} \text{Beibl. xii. p. 326.}$$

The values for Cu and Zn agree with the results of other experimenters, but that for Fe is too low. Bryström's value = $\cdot 112$ is taken. Naccari's values for c' have been used in each case.

reduced to about 1.16. The varnish was, however, slightly softened at the highest temperature, so that the character of the surface would change somewhat during the cooling.

The much larger square nickel-plated bars used by Mitchell in his repetition of Forbes's experiments on conductivity give for the cooling experiment $n = 1.26$ (see p. 441). This increase of n may be due to change of form of section, or to change in dimensions, as both these circumstances affect the stream-lines* produced in the air by the presence of the heated bars. It seems probable, however, that the only part of the loss of heat which is altered by alteration of the nature of the surface, is that part due to radiation.

From these facts we conclude that the loss of heat from an element of surface of heated bar, due to the effects of radiation, conduction, and convection into the surrounding air, is proportional to the n th power of the excess of temperature of that element above that of the surrounding air.

The fundamental equation for the state of heat along such a bar becomes then:—

$$c\rho \frac{\partial v}{\partial t} = \frac{\partial}{\partial x} \left(k \frac{\partial v}{\partial x} \right) - \frac{p}{q} \cdot hv^n. \quad . \quad . \quad . \quad (1')$$

It is evident from this equation what a great effect the outer conductivity has on the nature of the solution of the problem of motion of heat in a bar. The solution in terms of exponentials for the steady state used by Despretz†, Wiedemann and Franz‡, and others is replaced by a power of the temperature, and the solution for the “steady periodic” state first given by Ångström§ no longer holds. The above solutions neglect also variations of k , and we proceed to consider the effect of this.

Taking the case in which the temperature state is steady, we have the equation

$$\frac{\partial}{\partial x} \left(k_v \frac{\partial v}{\partial x} \right) - \frac{p}{q} hv^n = 0; \quad . \quad . \quad . \quad (4)$$

or taking k_v as a linear function of the temperature, thus,

$$k_v = k_0 + k'v,$$

we have, on expanding the equation (4),

$$(k_0 + k'v) \frac{\partial^2 v}{\partial x^2} + k' \left(\frac{\partial v}{\partial x} \right)^2 - \frac{p}{q} hv^n = 0. \quad . \quad . \quad (5)$$

* See for stream-lines, Lodge, Phil. Mag. xvii. p. 214 (1884); Rayleigh, Proc. R. S. Dec. 1882.

† *Ann. de Chim. et de Phys.* xix. et xxxvi.

‡ Pogg. *Ann.* lxxxix.

§ *Ibid.* cxiv., cxviii., cxxiii.

As the term in $\left(\frac{\partial v}{\partial x}\right)^2$ is often neglected in the mathematical treatment of conductivity*, it is interesting to compare its value, as deduced from experiment, with the first term of the above equation.

Taking Mitchell's figures for his iron bar, we deduce

	Value of 1st term.	Value of 2nd term.
At 50° C. excess. .	$\cdot 011 (5\cdot 5) = \cdot 06$	$\cdot 00001 (2500) = \cdot 025$
„ 100 „ .	$\cdot 011 (13) = \cdot 14$	$\cdot 00001 (12,000) = \cdot 12$

From which it is evident that the neglect of the second term will seriously affect the results, unless k' is very small, *i. e.* the conductivity nearly independent of the temperature. In the above case the conductivity has been taken as changing 10 per cent. in 100° C., which, according to the experiments of Forbes†, Kirchhoff and Hausemann‡, Lorenz§, and others, is by no means an extreme case.

Those determinations of conductivity which involve the assumptions $k' = 0$, $n = 1$, need not be considered, as the results derived from them can only be rough approximations. Integrating (4) with respect to x , between the limits x_1 and x_2 , we have

$$q/k_v \frac{\partial v}{\partial x} \Big|_{x_2}^{x_1} = \int_{x_2}^{x_1} p h v^n \cdot dx ; \quad . \quad . \quad . \quad . \quad (6)$$

or the difference between the amount of heat flowing along the bar at points x_1 , x_2 is the amount of heat lost from the sides between the two points.

If there be no “source or sink” along the bar except at the origin, and the bar be long enough to make $\frac{\partial v}{\partial x} = 0$ at its end, we have

$$q k_v \frac{\partial v}{\partial x} = \int_{\infty}^x p h v^n dx, \quad . \quad . \quad . \quad . \quad . \quad (7)$$

which determines k_v from observations of v along the bar.

* The effect of this will be to raise the value of the conductivity as deduced from experiment.

† Trans. R. S. E. xxiii., xxiv.

‡ Wied. Ann. ix., xiii. Kirchhoff and Hausemann neglect, however, the second term in equation (5).

§ *Ibid.* xiii.

We have then in (7) the solution of the problem of determination of conductivity by the bar method, free from the most serious of the errors involved in the usual assumptions. The outstanding assumption is that of plane isothermal surfaces.

To confirm the deductions made in this paper, from the fact that the loss of heat from a heated bar is proportional to a power of the temperature-excess, I proceed to apply them to the observations of Mitchell*, who has repeated Forbes's experiments on conductivity, after having the bars used by Forbes nickel-plated. He gives the values of $-\frac{\partial v}{\partial t}$ for different temperature-excesses for an iron bar; and from this the following Table is calculated:—

Temp. excess in degrees Cent.	$-\frac{\partial v}{\partial t}$	c_v specific heat. $c_0=1$.	$-c_v \frac{\partial v}{\partial t}$	$v^{1.26}$	$\frac{c_v \frac{\partial v}{\partial t}}{v^{1.26}}$
10	·11	1·01	·1111	18·19	·0611
20	·26	1·021	·2655	43·56	10
30	·43	1·031	·4433	72·61	10
40	·61	1·041	·635	104·5	10
50	·807	1·052	·849	138·4	14
60	1·00	1·062	1·062	173·9	07
70	1·19	1·072	1·276	211·2	05
80	1·405	1·082	1·521	248·9	11
90	1·605	1·092	1·755	289·8	06
100	1·83	1·103	2·018	331	10
110	2·04	1·113	2·270	373·4	08
120	2·28	1·124	2·563	417·6	14
200	4·4	1·206	5·307	792·5	·0670
250	6·12	1·258	7·699	1051	·0733

The constancy of the quotient $c_v \frac{\partial v}{\partial t} / v^{1.26}$ up to a temperature-excess of over 100° C. shows how well the index 1·26 represents the cooling in this case. Above 100° C. n appears to increase.

Putting $n = 1.26$ in (7), the equation determining k from the observation of temperatures along such a bar heated at one end is

$$qk_v \frac{\partial v}{\partial x} = \int_{\infty}^x p h v^{1.26} dx.$$

Mitchell gives v and $-\frac{\partial v}{\partial x}$ at different points along the bar, and from these we have the following Table:—

* Trans. R. S. E. 4 July, 1887.

x in feet.	v .	$-\frac{\partial v}{\partial x}$	$v^{1.26}$	$\int_{\infty}^x v^{1.26} dx$.	$\frac{\int_{\infty}^x v^{1.26} dx}{\frac{\partial v}{\partial x} \propto k_v}$.
.25	172.1	195.4	656.3	446	2.28
.5	125.25	132.2	439.7	311	2.35
.75	92.3	92	299.4	220	2.39
1.25	52.0	47.9	145.3	113	2.36
1.75	30.35	29.2	73.7	60.8	2.09
2.25	18.2	15.2	38.7	33	2.18
2.75	11.15	10	20.9	19.2*	1.92
3.75	4.3		6.16		
4.75	1.85		2.27		
5.75	.7		.96		

The numbers given in the last column indicate a rise of conductivity with temperature, which agrees with the result given by Mitchell as his most reliable. As the integration

$$\int_{\infty}^x v^{1.26} dx$$

is performed graphically, no great importance is to be attached to small variations of results. The number 2.28 ought to be increased about 5 per cent., as the index 1.26 in the cooling experiment only holds up to about 100° C. temp.-excess. A close agreement of the values of k with one another is probably not to be expected, on account of the deviation of the isothermal surfaces from planes caused by the insertion of the thermometers into the bar. The method of Forbes would be much improved in this respect if the temperatures were determined by thermo-junctions either set in the bar at different points, or movable, such as Wiedemann and Franz used.

Finally, then, in the general theory we have the equation of continuity in the form

$$c_v \rho \frac{\partial v}{\partial t} = \frac{\partial}{\partial x} \left(k_v \frac{\partial v}{\partial x} \right) + \frac{\partial}{\partial y} \left(k_v \frac{\partial v}{\partial y} \right) + \frac{\partial}{\partial z} \left(k_v \frac{\partial v}{\partial z} \right)$$

with the condition at surfaces in contact with gas

$$k_v \frac{\partial v}{\partial n_1} + h v^n = 0,$$

where n can only be taken = 1 when temperature changes of only a few degrees occur, but where c_v and probably k_v may be taken as constant when changes of temperature of not more than 50° C. occur.

* Liable to an error of about 5 per cent. on account of uncertainty of cooling &c. below 10° C. excess.

LIII. *The Constitution of the Aromatic Nucleus.* By S. A. SWORN, B.A., Assoc. R.C.Sc.I., late Brackenbury Scholar of Balliol College, Oxford.

[Concluded from p. 415.]

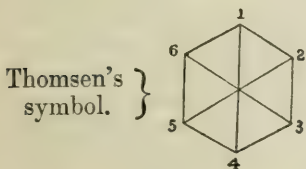
II. Arguments against Meyer's symbol.

(1) **B**AEYER has brought forward, as an argument against Ladenburg's symbol, the fact that dihydroxyterephthalic ether (a benzene derivative) is converted by nascent hydrogen into succino-succinic ether (a hexamethylene derivative) (*Ber.* xix. p. 1797).

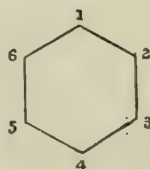
A. K. Miller and Ladenburg have each shown his deduction to be faulty (*J. C. S.* 1887, p. 209). (Without a single exception the facts quoted by these chemists can be as well explained by the symbol of Claus as by that of Kekulé.)

It would indeed be a serious objection to Ladenburg's symbol could it be shown that on hydrogenation the para-linkages are successively split. We should thus expect to get two trimethylene rings. Facts are in opposition to such a supposition.

But this is not the only conceivable way in which the reaction may occur. A hexamethylene ring may be formed by the dissolution of one para- and two meta-linkages, the atoms being supposed to open out into a hexagonal ring (*vide supra*, p. 405). It is perhaps improbable that a stable meta-linkage (corresponding to ortho-linkage in Thomsen's symbol) would be severed in favour of a much weaker para-linkage. The fact, however, remains that the additive compounds can be so derived. The hexamethylene ring so obtained will differ from that derived from Thomsen's symbol in this respect—that the order of the six atoms will not be the same. This is evident from the following diagrams:—



will give



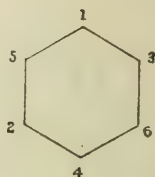
Ladenburg's
symbol.



will give



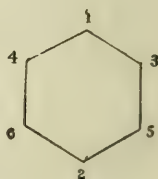
opening out to



or



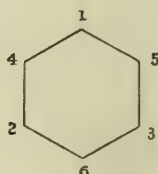
opening out to



or



opening out to



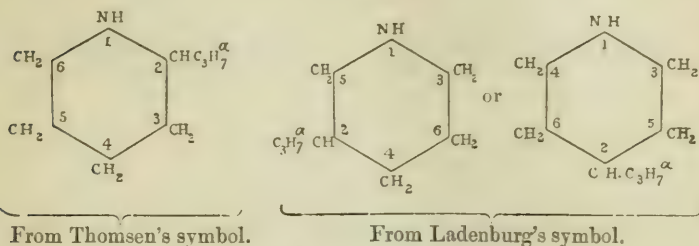
The difference may be thus defined:—

The symbol of Thomsen will give rise to a ring in which each carbon atom is bound to what were in the benzene molecule its ortho-neighbours; whereas in those derived from Ladenburg's symbol, a carbon atom would in no case be directly attached to atoms which were previously its ortho-neighbours.

The ascertained constitution of conine helps us to distinguish between these two methods of notation. Conine is undoubtedly the hexahydro-derivative of ortho-propyl pyridine. (See especially the researches of Hofmann, Ladenburg, Skraup, and Cobenzl, on conine and picolic acid, described in Pictet's "*La Constitution Chimique des Alcaloïdes Végétaux*.")

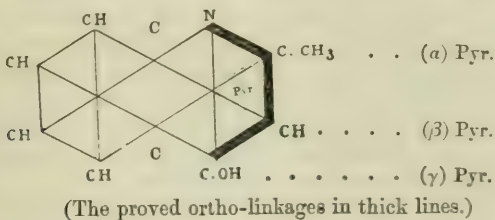
Its optical activity is due to the presence of an asymmetric

carbon atom. No exception to this rule is at present known (see especially "*Dix années dans l'histoire d'une théorie*," by Van't Hoff). When derived from Ladenburg's symbol, the formulæ for conine cannot possess an asymmetric carbon atom. This is evident from the following symbols:—



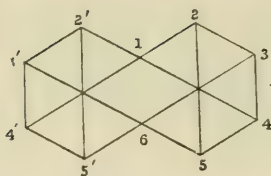
Moreover Ladenburg's theory would indicate the possible existence of two position isomers derived from α -allyl pyridine.

The properties of conine, when thus considered, afford direct evidence that pyridine is to be represented on Thomsen's type and not on Ladenburg's. This deduction is confirmed by the fact that Knorr and Antrick's researches (*vide supra*, p. 414) prove by direct synthesis the existence of ortho-linkage between the nitrogen and the α -, β -, and γ -carbon atoms in the pyridine nucleus of γ -oxy-quinaldine, thus:—



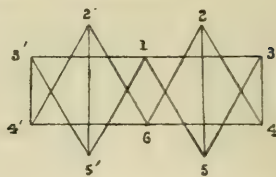
(2) It is commonly stated that Ladenburg's symbol will not account for the conjugated derivatives of benzene. This is not altogether true, although there are difficulties which do not exist in the case of Thomsen's symbol.

In the case, for example, of naphthalene it is necessary to sever two para-linkages and to set up an ortho-linkage in each aromatic nucleus (II.). The symbol (III.) which Meyer gives is complicated by the improbable supposition that the atoms 3 and 3', 4 and 4', are directly linked together, and by the fact that phenanthrene cannot be similarly represented (*vide infra*).

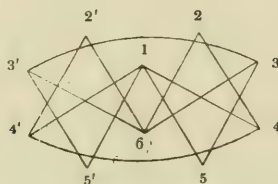


From Thomsen's symbol.

I.

From Ladenburg's symbol
(octahedral).

II.



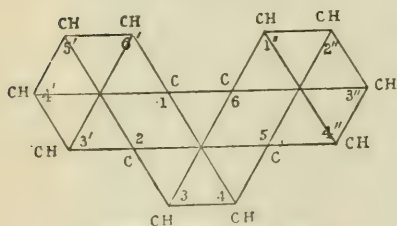
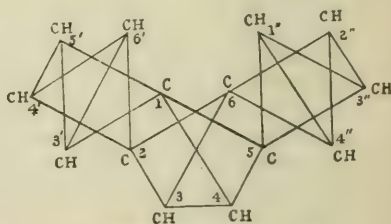
Meyer's symbol.

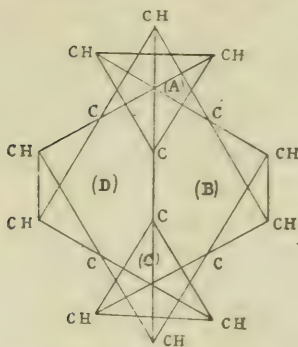
III.

Naphthalene as thus constituted (II.) could hardly be said to consist of two aromatic nuclei. That it does consist of two such nuclei is evident from the fact that, like benzene, it has an abnormally low molecular volume, and gives substitution products by direct nitration and sulphonation. Moreover, in the formation of these derivatives the action appears to follow as closely as possible the laws of substitution which hold for benzene derivatives. Hartley found the absorption of the ultra-violet rays characteristic of benzene to be even greater in the cases of naphthalene, anthracene, and phenanthrene.

The same difficulties hold with regard to the other conjugated bodies. Unless ortho-linkage is set up it is difficult to account for the ascertained constitution of phenanthrene, more especially its formation from stilbene.

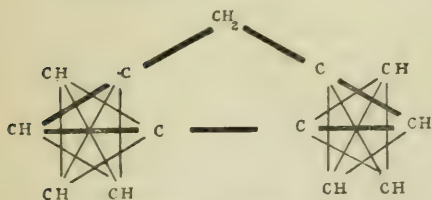
I may quote the formulæ which might be assigned to phenanthrene and pyrene.

Phenanthrene, $C_{14}H_{10}$
(from Thomsen's symbol).Phenanthrene, $C_{14}H_{10}$
(from Meyer's symbol).

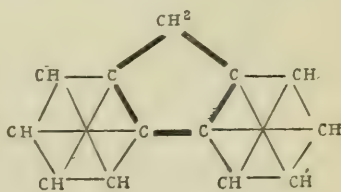


Pyrene, $C_{16}H_{10}$ (from Meyer's symbol).
Compare with p. 416.

(3) It is generally admitted that closed chains of more than six atoms do not exist. If this be so, it is difficult to account for the constitution of fluorene on Ladenburg's theory, as it is then necessary to assume the existence of a closed chain of seven carbon atoms. In the formulation of this body from Thomsen's symbol this ring consists of five carbon atoms only. The relationship of fluorene to phenanthrene, through diphenylene ketone and diphenic acid, proves it to be a di-ortho-compound. The argument is evident from the following symbols:—



Fluorene, $C_{13}H_{10}$
(on Ladenburg's symbol).



Fluorene, $C_{13}H_{10}$
(on Thomsen's symbol).

In this connexion I may quote the following passage from Watts' 'Dictionary of Chemistry' (new edition, i. p. 300):—
"Ortho-compounds readily give rise to products of condensation in which the side chains may be supposed to be joined in the form of a ring; this tendency is observed to some extent in the para-series but not at all in the meta-series."

(4) The analogies between the ortho- and para-derivatives of benzene as opposed to the meta- have been cited by Koerner (J. C. S. 1876, i. p. 240) and by Lellmann (*Ber.* xvii.

p. 2720) in favour of ortho- and para-linkage. It must be confessed that these arguments depend upon hypothetical views as to the cause of such analogies, and are therefore not very conclusive. Stuart has similarly brought forward the results of his experiments on the benzolmalonic acids (J. C. S. 1886, p. 357) in support of Kekulé's formula—a formula which is at variance with so many facts.

III. *Thomsen's Symbol.*

The preceding discussion affects merely the nature of the atomic linkage, and shows that the diagonal symbol of Claus is alone consistent with all the facts. The argument is not merely based on hypothetical analogies between benzene and pyridine derivatives. The independent consideration of these groups of bodies clearly shows that they must be formulated on the same type.

(1) Thomsen's symbol is a development of that of Claus, but the positions of the hydrogen atoms are not considered.

It is most natural to suppose that any given hydrogen atom is attached to its carbon atom, in such a manner that the direction of the valency falls within the solid angle formed by the three other valencies which unite that carbon atom to its ortho- and para-neighbours. If, for example, we assume that this valency is equally inclined to the other three, it will make an angle of $148^{\circ} 36'$ with them (or $31^{\circ} 24'$ with the diagonal of the octahedron).

But whatever may be the true angle of deviation it is evident that, whilst the meta- and ortho-carbon atoms are equidistant, the meta-hydrogen atoms (or rather their mean positions) would be closer together than the ortho-.

If the configuration of the benzene molecule as a whole were octahedral, we should expect the crystals of benzene to belong to the regular system. But they are rhombic. Schrauff has considered this point (Wiedemann's *Annal.* Neue Folge, xxxi. p. 540), but the positions which he assigns to the hydrogen atoms give a symbol which would indicate the existence of two isomeric mono-substitution derivatives.

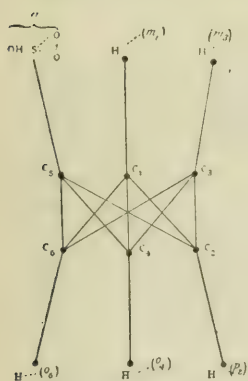
Further, any space formula for benzene, which represents all the atoms in one plane (see Claus, *Ber.* xx. p. 1425), would lead us to suppose that benzene would crystallize in the hexagonal system.

(2) The development of Thomsen's symbol which I have proposed in no way affects the questions which were previously discussed (I. and II.).

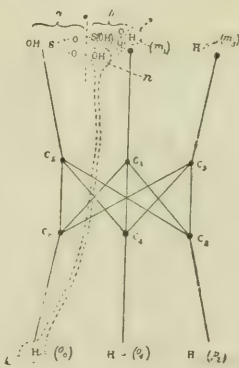
It affords a basis for an explanation of the so-called para-

and meta-laws of substitution (see Armstrong J. C. S. li. p. 259).

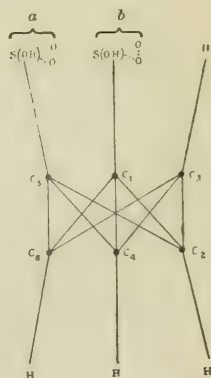
(a) Let us consider the continued action of sulphuric acid upon benzene. The sulphonic group first introduced is itself sulphonized, and for the moment an unstable body (B) is formed. This compound gives off a molecule of water, the hydroxyl (*n*) being eliminated with one of the ortho- or meta-hydrogen atoms (o_4 o_6 m_1 m_3). Simultaneously with this change the two sulphonic groupings (*a* and *b*) become disunited, and (*b*) takes the place of the eliminated hydrogen atom. As in a large number of such molecules, the hydroxyl (*n*) will be more often in closer proximity to a meta- than to an ortho-hydrogen atom, and always closer to one of these than to the para-hydrogen atom (*p*), we shall get the formation of meta- and ortho- to the exclusion of para-benzene-disulphonic acid, and of these the meta will be the chief product. These changes may be graphically represented thus:—



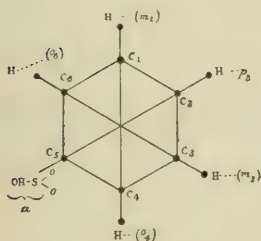
(Elevation.)



Intermediate Body (B).



Benzene meta-di-sulphonic acid.



(Plan).

Benzene sulphonic acid, $C_6H_5SO_3H$.

(b) This explanation is dependent upon the nature of the radical first introduced. If this radical has no tendency to form addition products, the further substitution must be effected by the momentary dissolution and re-establishment of one of those aromatic linkages which attach its ortho- and para-neighbours to the carbon atom whose hydrogen is already displaced. (This explanation is admitted for the mono-substitution derivatives.)

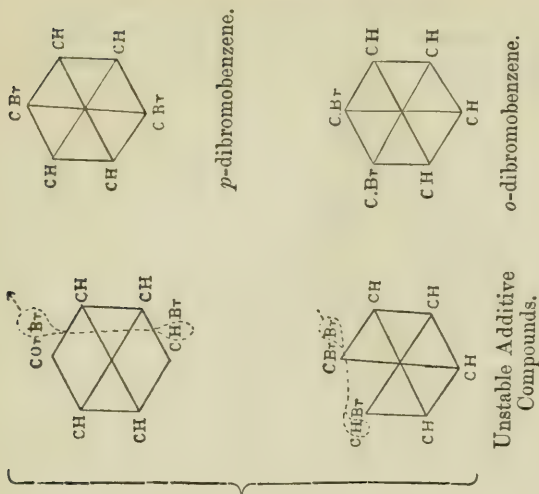
Meta-derivatives are not obtained because there is no meta-linkage, and para-derivatives are formed in greater quantity than ortho- because the para- is less stable than the ortho-linkage. The formation of *o*- and *p*-dibromobenzenes, by the direct bromination of benzene, may be thus represented. (See page 451.)

(c) In a similar development of Meyer's symbol the ortho-hydrogen atoms would be nearest to one another, and similar arguments would lead us to ortho- and para-laws, and the formation in each case of meta-compounds in small quantity.

Attempts have been made by Schiff (*Annalen*, cexx. p. 303), by Lossen (*Annalen*, cexxv. p. 119), by Horstmann (*Ber.* xx. p. 766; xxi. p. 2211), by Brühl (*Annalen*, cc. p. 228), by Thomsen (*Therm. Unt.* iv. pp. 61, 273), and others to determine the constitution of benzene from its physical constants. The calculations of these chemists presuppose the existence of paraffinoid and olefinoid linkages alone, and lead to conflicting results. Moreover they are based on so-called "laws," such as that of Kopp, the general application of which (in the case of molecular volumes) has been disproved by numerous researches.

It is certainly true that the linkages in aromatic compounds are not directly comparable in such a manner with those in fatty bodies; and it is therefore impossible to make use of arguments based upon the measurements of specific volumes and refractive indices and upon thermochemical data, until some quantitative connexion has been made out.

In a future paper I hope to bring forward some calculations based upon space formulæ and made with the object of advancing this question.



LIV. *The Measurement of High Specific Resistances.* By
R. THRELFALL, M.A., *Professor of Physics, University of
Sydney, N.S.W.**

[Plate XIV. fig. 3.]

THE experiments which form the subject of this paper were begun almost immediately after my arrival in New South Wales, in June 1886, and have been continued at intervals ever since. The original object was to measure accurately the resistance of certain gums produced by trees growing in the Colony. The only gum thoroughly examined however, up to now, is that produced by the "grass tree" (*Xanthorrhœa hastilis*). This gum, in spite of many attempts to improve it by various methods of purification and by mixing with other substances, turns out to be useless as an insulator, having in fact no higher resistance than, say, ordinary samples of resin, that is about 4.1×10^3 megohms per cubic centimetre. Besides this, the gum in question is faulty in other ways. It is of the nature of shellac, but cannot compete with the shellac as ordinarily supplied either in price or purity. In addition it has the two fatal defects of being partly soluble in water and of decomposing before it melts. Long-continued gentle heating does not seem to improve it in this latter respect; while the texture of the material becomes looser, it grows friable and very dark in colour. Benzoic acid appears to be given off in large quantities during the process. In spite of many attempts I have hitherto failed to obtain any considerable quantity of the fig-tree gums which are said to be produced in large quantity in the northern parts of the Colony. This paper therefore will be devoted to a description of the method adopted in measuring these resistances, a method which ultimately reached a considerable degree of perfection; partly on account of the modifications introduced in the construction of high-resistance galvanometers, and partly on account of the peculiar property of Clark cells. The method has since been employed for other measurements, as will be shown later on.

I do not wish to claim any superiority for the galvanometer over the electrometer methods, except that, given the galvanometer and cells, it is certainly more easily applied, especially when the determinations are numerous. I was forced to adopt the galvanometer method in this instance through not having an electrometer; but I was by no means

* Communicated by the Physical Society: read March 23, 1889.

unwilling to do so, because I have long been of the opinion that the galvanometer as usually constructed is susceptible of considerable improvement for delicate work by simply pushing the ordinary conditions of sensitiveness nearer to their limit.

GENERAL DESCRIPTION OF METHOD.

Measurement of Resistance of Gums.

The substance to be tested forms a layer of very exactly estimable dimensions between two plates of conducting material. The experiment consists in obtaining equal deflexions of a sensitive galvanometer—(1) when a known fraction of the E.M.F. of a Clark cell is allowed to act through a megohm in the galvanometer circuit; and (2) when the E.M.F. of a known number of compared Clark cells is allowed to act through the resistance to be measured.

The apparatus, therefore, consists of the plates with the substance to be investigated, the galvanometer, the standard cells, and a megohm.

The Resistance-plate Arrangement.

This consists of two brass plates accurately rectangular and scraped flat on one surface. The dimensions of the plates I used were—length 15·2 centimetres, breadth 12·7 centim., thickness 1·6 centim. These rather exact numbers were obtained by filing. The measurements were made (α) by beam compasses, (β) by the dividing-engine. Neither the corrections which had to be introduced for temperature nor the comparison of the dividing-engine scale and the beam compasses with the standard metre are given, as no absolute measurements of pure substances have been made. Several measurements of each plate were made by both methods. The surfaces were made flat by scraping, and this process was continued till the contact was sufficiently perfect for one plate to lift the other when laid on it, both surfaces being clean in the ordinary sense. The upper plate is furnished with a solid handle something like the handle of a flat-iron, and is pierced by three holes, through which pass the micrometer distance screws (see Plate XIV.). The screws are 4 centim. long, and the threaded portion is ·55 centim. in diameter (they would have been better if twice the diameter). The micrometer-heads are divided into a hundred parts each, and the mean pitch of the screw, as determined by a comparison with the millimetres of a standard scale by means of a measuring microscope, is 39·5 divisions to a millimetre; that is, one turn is equal to ·5063 millimetre at 20° C. The points of the

screws are conical, and the distance from the end of the thread to the point of the screw is 5 centim. The screws are of steel tempered to the blue, the heads of brass; and the tap used to produce a thread in the holes through the brass plate was identical with one of the screws; the lathe being set to use the same part of its screw and of its change wheels during the making of each; the measurement showed that the screws were very good especially in the middle portion; they were also practically exactly alike. The goodness of the fit in the brass plate was shown by the fact that an increase of temperature of 20°C. was sufficient to "bind" the screws very perceptibly. Indices similar to the indices of spherometers were erected in the upper plate of the apparatus—one index for each screw. All the workmanship being accomplished, the plates were next platinated by a process given in Gore's 'Electro-metallurgy' under the name of "Roseleur's Process;" a previous experiment showed that when the directions are faithfully carried out, this process will yield a hard bright deposit of platinum. The bath, however, is very troublesome to keep in order since no solution of platinum takes place to supply the place of that deposited. To prepare the plates for platinating, they were first heated to the temperature of boiling water and rubbed on the scraped surfaces with a solution of caustic potash. Finally, they were rubbed with a bit of fine pumice dipped in dilute caustic potash. This is by far the best laboratory method known to me for preparing surfaces of brass for electro-plating; a clean surface is obtained with comparatively little abrasion. Before I found this out, I was much troubled to secure a good deposit. After platinating, care being of course taken to prevent the deposit being unequal, the plates were carefully washed and dried. It was noticed that the metallic surfaces were covered with a faint bloom of black platinum. On placing the plates together and moving the top one slightly, the lower plate at once adhered, and though weighing several pounds could easily be lifted by the upper one. On pulling the plates apart the "bloom" was found to be burnished practically all over both surfaces, showing of course that the platinating had not sensibly altered the planeness of the surfaces. For this accuracy I am much indebted to the university assistant, Mr. James Cook, who, being accustomed to prepare optically flat surfaces, was led by the application of experience gained in that way to the happy result above mentioned.

The exact position of the two plates with respect to one another was secured by cementing the plated surfaces together with hard paraffin. Two lugs of thick brass were

made fast by screws to each plate, and brought as near to one another as consistent with leaving an air insulating space between them. These were then bored to fit slightly conical steady pins. A rim was also screwed round the lower plate, so that when the top plate was removed the bottom plate resembled a tray. The rim projected about $\cdot 8$ of a centimetre on each side of the surface, and extended to a height of about half a centimetre above it.

The gum was introduced between the plates in the following way. The micrometer-screws were carefully cleaned and screwed through their holes till they made contact with the lower plate; the point of contact was almost as easy to estimate as in the ordinary use of the spherometer. At all events three or four consecutive attempts to fix the point of contact did not differ from each other by more than about one half of one of the micrometer-divisions. The accuracy with which the contact-point can be fixed depends mainly on the workmanship of the screw, which must fit perfectly "tight and free," to use the mechanic's very expressive term. The contact-points having been found, they were permanently scratched on the micrometer-heads and called zero points. The next operation consisted in screwing each of the screws through a known number of turns. In one experiment this amounted to making the distance of the plates apart = $\cdot 02$ centim.; and in another the distance was reduced to $\cdot 01$ centim. with equally good results. It is not advisable in any absolute measure to reduce the distance to much less than this, because the error of the micrometer, depending as it does (as in this case) chiefly on the small irregularities of the screw, must not be allowed to become sensible. There is no doubt, however, that with first-rate appliances the micrometers might be easily made a hundredfold as accurate as mine, and their travel actually measured *in situ* by a suitable reading-microscope. In this case it would be important to make the screw portion much thicker to avoid any risk of permanent distortion (twisting) when the screws are finally screwed back while partly held by the gum*.

The screws being adjusted, the plates are slowly heated in a gas-oven till some gum laid on the surface of the lower one is in a state of quiet fusion. The great object is to avoid any distortion of the plates. With this aim in view the plates described were cast about twice as thick as they finally required to be in order that the "shell," supposed to be in a

* Since this was written an improvement of the above micrometer-screw has been devised, entirely getting rid of the difficulty here referred to.—Oct. 3, 1889.

different state of strain to the interior, might be approximately removed. The plates were planed first on one side and then on the other till the right quantity of metal was removed; the last cuts being taken very fine. The handle affixed to the upper plate was of course arranged so as to fit loosely, and not in any way constrain the free expansion and contraction of the plates; the temperature having been often violently changed (by heating for cement &c.), it is hoped that the plates may be considered fairly well annealed. There is no doubt, however, that for complete satisfaction in an absolute measurement the plates should be capable of being optically examined during the process of heating. This would require to be done in the gas-oven or other uniform field of temperature and at the time the experiments were made. I had not the requisite appliances.

It was found by several trials that the best way of obtaining a layer of gum free from bubbles between the plates, was to float the lower plate or tray pretty full of gum, and also to obtain a layer of gum free from bubbles and in quiet fusion on the upper plate. In the case of the grass-tree gum this could only be obtained by heating the gum for some time at a temperature higher than the one at which it was when the plates were brought together. This was accomplished much in the same way as is sometimes done in microscopy—when the cover-slip is placed on the slide with one edge down and the other end gradually lowered. The freedom from bubbles of the layer of gum obtained in this way was tested by allowing the plates to cool, and then heating the lower one till its surface reached the temperature of the melting-point of the gum, the upper one being kept cool. This being done the upper plate could finally be lifted, leaving only a small portion of the gum on the lower plate—owing to the small heat-conductivity of the gum. The layer of gum was left thick on one occasion for the purpose of this test, and when the manipulation described above was properly carried out there were no bubbles; the layer in fact was very homogeneous indeed. The only danger left so far as the insulating material is concerned is that it may tend to crack away from the plates during cooling. In the cases examined this did not seem to be the case, because in the first place a thick layer of the same substance rapidly cooled on a thin plate showed no tendency to crack. Again, a great many insulating substances are more or less plastic, or rather viscous, down to temperatures very nearly approaching those at which experiments are usually made (in this country 23° C. is a not unusual temperature). Again, the massiveness of the plates being

considerable no distortion of them ought to be caused by the gum, if the ordinary precaution of allowing them to cool slowly be observed.

In the experiments hitherto made the cooling took place during the night in the gas-oven, which being coated with non-conducting material took a long time to cool. On the other hand, in all experiments of the sort one is in a dilemma. If the substance is placed between conducting-plates there are dangers of the kind mentioned; if, on the other hand, the material itself be worked with a view to making it take a prescribed form, the difficulties, especially in the measurement of its thickness, become great. I began by making some attempts of this kind, using blacklead to make the gum-surface conducting, and plating this electrolytically. The difficulties arising in the shaping of the plates are, however, practically prohibitive with friable material. Besides this I have often noticed that in electrotyping it is difficult to prevent the deposit being "spotty" at first, and this has shaken my faith in the perfect continuity of ordinary blacklead surfaces. Possibly platinating with an induction-coil may be really the best way.

However, to finish the description of the case in point:—The gum was carefully scraped away from the edges of the plates as soon as they were cool, and the screws were screwed back. This could not be done with any ease at first because of the cementing action of the gum. This was got over by heating the head of the screw with a Bunsen flame: finally, the screws were retracted far enough to be quite out of the way*. A correction to the area of surface has of course to be made for the three screw-holes. If the thickness of the gum be considerable compared with the diameter of the hole, this may be very complicated. In the present case it was negligible. Thus the whole area of the gum-plate was 193.04 centim. less the area of the three screw-holes — 7128 centim., *i. e.* 192.3272.

Now it is clear that, owing to the curvature of the lines of flow round the edges of the holes, the real correction will be less than the one made. The deposit of gum, however, is pierced by a hole corresponding to the conical end of the screw and, consequently, only very small.

* NOTE, Dec. 1888.—The difficulty is, however, serious, and has led to new hollow screws being made. Through the holes bored down the centres of the screws gold-plated copper rods pass; these are pinned to the screws till it is required to retract them; the ends of the screws themselves are flush with the lower surface of the upper plate, or very near so.

Finally, the steady pins are taken out of the plates and the gum is ready to be measured.

In order to measure the resistance of good insulation by means of this arrangement, it is clear that it will be advantageous to have a galvanometer of the highest degree of sensitiveness. This is desirable both because the thicker the insulating layer the less will be the experimental error in the determination of its thickness; and the smaller the electromotive force required the less will be the difficulty of estimating it exactly, as will be shown in the proper place. I therefore attempted to obtain sensitiveness by pushing the ordinary conditions further than is usually done. My first experiments were on a galvanometer of about 9000 ohms' resistance, made by the Cambridge Scientific Instrument Company. It was soon very evident that when the current reached the value of about 10^{-7} ampere, the torsion of the suspension became important. My first modification was to increase the length of the fibre to about 12 inches; this led to considerable difficulty of adjustment, but increased the sensitiveness about fifty-fold. It then became clear that the next step must be to get the magnets more perfectly astatic and to reduce the weight of the mirror. The reducing of the weight of the mirror turned out to be more difficult than I anticipated; however, it was finally arrived at, and at the same time the astaticism was made more perfect. Some experiments showed that it was very difficult to get two sets of steel bars of the kind ordinarily employed even reasonably astatic. The difficulty lies partly in the magnetizing and partly in obtaining exactly equal quantities of steel in the two systems of magnets. In fact it is necessary that the steel bars be magnetized *in situ*, otherwise they can hardly be perfectly arranged and are sure to demagnetize each other more or less. Now when the two magnet systems are only separated by a bit of aluminium, say three inches long, it is impossible to thoroughly magnetize one system without demagnetizing the other more or less. Consequently it is necessary to set up an arrangement so that both systems can be magnetized at once. The following is a description of the arrangement adopted:—It consists of two small electromagnets with extremely soft cores, and movable pole-pieces most carefully worked so as to fit the ends of the cores. Every precaution was taken to make the electromagnets as much alike as possible; the iron was cut off the same rod, it was bent to the same templet, the annealing of both cores was done in a box of asbestos at the same time. The four brass bobbins

carrying the wire were also made as much alike as possible, and the same number of turns of wire were put on each bobbin by means of a revolution-counter. The winding was quite uniform, No. 18 B.W.G. wire being used. As a check the resistances of the bobbins were measured, when it fortunately happened that two were about one half per cent. higher than the other two, and so they were paired. The wire, it need hardly be added, was wound on to the four coils under a constant strain. The two electromagnets were then mounted on a permanent stand—one being kept steadily in a fixed position, and the other being capable of sliding parallel to a line drawn perpendicular to the lines joining the centres of the poles of each magnet. The pole-pieces were bevelled off from the top side; but the area of the ends remained large compared with the size of the magnets to be magnetized. The condition as to equality of quantity and quality of steel in the galvanometer magnets was next considered. After some reflexion I decided that the most probable way of securing equality would be to discard bar-magnets entirely and use disk-magnets. I therefore procured a small piece of sheet steel about as thick as ordinary thin writing-paper, and had a die constructed so as to stamp small disks from this sheet. The sheet was fairly hard, and it was found that the disks "stamped" better when the sheet was taken in its natural state than when it was softened. A considerable number of disks were stamped out of the sheet, and these were then laid on a bit of flat iron and raised together to a bright red heat; they were then plunged together into a jar of cold water. On examination they all seemed to be glass-hard, and some of them remained flat. The four flattest ones were chosen and prepared for mounting. A bit of aluminium wire was cut to the right length and beaten out flat at each end. The disks were then cemented with shellac varnish, one on each side of each flattened end of the aluminium wire. The wire was thus much more accurately the centre of rotation of the magnetic system than is generally the case. Attempts were then made to get a good light mirror. About three ounces of small microscope cover-slips were examined by aid of the reflected image of the bars of a window, and from these about twenty were selected and silvered by the Rochelle salt process. They looked very good, but on mounting for trial without strain they all turned out disappointing. I finally made use of a small portion of a larger mirror that had got broken. This was cemented on to the flat surface of one of the steel disks and was found not to be sensibly distorted. After trying very many cements, I

incline to think that nothing is better than a trace of slow-drying white paint. Amongst other experiments I tried cementing two very thin glass disks together, selected so as to mutually correct each other by the drying of the cement. I also tried a method of using plaster of Paris. As plaster of Paris expands on setting I covered the back of a thin mirror with a layer of it about $\frac{1}{12}$ in. in thickness on drying, this of course forced the mirror into a concave form. The back of the plaster was rubbed away on a fine file till it was only about $\frac{1}{20}$ in. thick, and the mirror still remained very concave. Since cementing magnets on to disks with shellac varnish invariably forces the mirror to become convex, I hoped that I should obtain a correction of the concavity produced by the plaster by the convexity which the shellac tends to provoke. My anticipation was completely realized; the mirror on examination turned out all that could be desired, but, alas, was too heavy for the purpose for which I required it. I can, however, most strongly recommend the process to anybody who desires a mirror to be flat and does not mind it being heavy. The best way is to use very little plaster and then to leave the mirror concave. This concavity can be removed by painting on small successive films of shellac; it must be remembered that shellac films go on contracting for several days after they cease to be sticky. Equally good results can of course be obtained by cutting out (with a rotating tube and emery) disks of the size required from previously examined thin sheet glass. The surfaces generally require regrinding. The advantage of the process described is that it enables thin cover-slip glass, which is generally to hand, to be kept flat.

The mirror having been mounted on the steel disks, these last were magnetized by the apparatus mentioned above. In carrying out this operation the following precautions have to be observed:—

1. The distance between the pole-pieces requires to be the same for each magnet. This was attained by setting them to touch a carefully prepared brass rectangular bar.

2. To annul the effect of any small outstanding differences between the cores of the magnets the current was supplied to them in multiple arc, and was strong enough to magnetize the cores beyond the saturation point.

The approximate moment of inertia of the magnet system was easily calculated, and it was found that the astaticism was at least ten times as good as the best I had been able to obtain with small bar-magnets mounted on mirrors or mica, and magnetized with a small horseshoe magnet. Of course,

as has been pointed out by several observers, there is danger of rapid variation of the magnetization ; but it was thought better to risk this than to force it by artificial " ageing " by heating or otherwise. As will be seen hereafter, the magnets were never exposed even during the experiments to anything but the smallest electromagnetic forces, and the controlling magnet was weakened and introduced from high above the galvanometer, and only lowered sufficiently just to make its influence on the combination really felt. This is a delicate operation with ordinary arrangements, but becomes simple when the construction of the galvanometer is modified in a way to be explained directly. If one desires to keep the astaticism perfect, it is necessary to be mindful not to use the controlling magnet so as to produce demagnetization ; nor must the currents through the galvanometer ever rise to much greater values than those corresponding to the effects to be observed. From an examination of the investigation in Maxwell, vol. ii. articles 437 and 438, both Mr. Adair and I came to the conclusion that the disk form of magnet would retain its magnetization pretty well. This has turned out to be the case, for after more than a year's hard use, for all sorts of purposes, the galvanometer has still a sensitiveness of about one division for 10^{-9} ampere. The galvanometer is in daily use for testing cells with a view to their application to the resistance measurements at present under discussion.

This galvanometer, however, never came to be relied on to measure currents of less than 10^{-8} to 10^{-9} ampere. In the course of reading on the subject I consulted the paper by Messrs. T. and A. Gray in the Proc. Roy. Soc. 1884, vol. xxxvi. p. 287. These gentlemen made use of a new arrangement of magnets and coils, which, however, can hardly be understood without referring to the picture, *loc. cit.* The coils and magnets were so arranged that the poles of the magnets were normally situated in conical holes containing the axes of the coils. The two horseshoe magnets were suspended from a frame of aluminium wire by one or two silk fibres of considerable length. The coils themselves were composed of very fine wire and had a high resistance. I lost three months' hard work in making and testing this arrangement, which certainly has the advantage of being practically perfectly astatic. The suspended arrangement being rather large was most troublesome to mount and balance, and had the additional disadvantage of having so great a moment of inertia that its period of vibration often amounted to 70 or 80 seconds. This sluggishness had the property of making it most difficult

to use, for it was hardly possible to distinguish the motion due to the electromagnetic forces from the never-ceasing motion due to air-currents. Though the instrument was well-protected by a glass case, and this generally supplemented by a wooden box, I never succeeded in eliminating the effect of air-currents, though it must be added I never succeeded well enough with it in other respects to make it worth while to apply the "subjective" method of mirror observation. In any case it seems to be essential to have a means of adjusting the coils to the magnets as well as the magnets to the coils; but the adjustments are very tiresome even with the facilities which in the later forms of this instrument I had for making them. My coils had not quite so much wire as those of the Messrs. Gray because I used all I had and could get no more in Australia. However, the aggregate number of turns amounted to 59,900, and the resistance at 20°C . was 15,852 B.A. units as against 62,939 turns, and a resistance of 30,220 ohms attained by the authors quoted. The authors also state that the wire was approximately uniformly distributed throughout their coils, though in my case this was found impossible, keeping the external dimensions quite constant, and therefore there was a slight difference between the coils, which, however, was compensated for by their arrangement in the instrument. I do not think that the diminution of the number of turns had much effect, because the diameter of the coils had reached 5.8 centim. Feeling that my non-success was probably to be traced to my inferior skill as an experimentalist, I undertook a long series of trials with a view to discovering the best way of hardening the magnets and their best position in the coils when at rest; amongst other experiments the following will do for description. Three bits of Stubb's steel wire were carefully cut and filed to a uniform length of $4\frac{1}{16}$ in., their diameters being $\frac{3}{64}$ in. These will be called A, B, and C. A was made glass-hard throughout; B was hardened through a distance of from $\frac{3}{4}$ to 1 inch at each end; C was hardened from a distance of from $\frac{1}{8}$ to $\frac{1}{4}$ inch at each end. These bits of wire were then magnetized by being placed between the massive poles of a very large electro-magnet. The cores of this magnet were 3 inches in diameter and about 25 inches long. The pole-pieces were very broad and thick. The magnetization of the steel was carried nearly to saturation, and the magnets were found by filings to be free from consequent poles. On taking the times of vibration the following numbers were obtained :—

A made 25 vibrations in 125 seconds.

B " " 124 "

C " " 105 "

After remagnetizing the magnets with about half as much current again round the electromagnet, it was found that

A made 25 vibrations in 123 seconds.

B " " 107 "

C " " 107 "

B therefore was improved; A and C remaining about the same. The deviations were probably produced by the unavoidable shaking and jarring in mounting the magnets, though this was done with considerable care.

These magnets were next observed with respect to their behaviour with one of the coils. Coil "No. 3" was selected for this purpose. It was placed on the table, and above it hung a specially fine spring-coil of wire forming part of a Jolly's balance. The magnets were hung from the end of this spring by a loop of silk, and could be adjusted to penetrate the coil to a greater or less extent. In general five positions were taken:—

Position 1. Magnet-end flush with the upper windings of the coil.

„ 2. Magnet-end at $\frac{1}{4}$ of the length of the hole through the coil.

„ 3. Magnet-end at the centre of the coil.

„ 4. Magnet-end at $\frac{3}{4}$ of the length of the hole through the coil.

„ 5. Magnet-end flush with the bottom of the coil.

Three Leclanché cells were allowed to run for four hours through the coil before the experiment began. The coil had a resistance of 3130 ohms.

First, a series of observations was taken by observing on the glass scale the equilibrium position of the magnet with the current direct and reversed. The magnet was then lowered to position 2, and so on; then it was reversed and the experiments repeated. Two complete sets each way were made for each magnet, *i. e.* forty observations of distance and forty reversals of current. The experiment was a very pretty one, and I never remember to have seen any apparatus work better. The following set is given as a sample from the notebook. The numbers, of course, have no significance except with respect to the actual coil and magnet taken.

Magnet B.

Position in Coil.	Double displacement with current + and -.	Number of experiments.
At edge of coil . . .	·43 centim.	3
$\frac{1}{4}$ way in	·77 "	2
$\frac{1}{2}$ "	1·03 "	2
$\frac{3}{4}$ "	1·159 "	2
Near bottom of coil . . ($\frac{1}{8}$ inch off)	1·04 "	2

Magnet B reversed, otherwise everything the same.

Position.	Displacement current + and -.	Number of experiments
At edge	·415 centim.	2
$\frac{1}{4}$ way in	·78 "	2
$\frac{1}{2}$ "	1·085 "	2
$\frac{3}{4}$ "	1·27 "	2
$\frac{1}{8}$ off bottom	1·11 "	2

The nett result was that all the magnets behaved best when they started from three quarters to the whole way in, and that B was best, C almost as good as B, and A distinctly the worst. On reducing the observations it turned out that the ratio of the mean displacements of A and B was about ·782, while the ratios of the magnetic moments was ·755. This relation is of about the order one would expect, seeing that the length of the magnets was about 9·6 centim. and the dimensions of the coil were :—axial 3·8 centim., radial at one end 2·4, radial at other end 2·5. The external surfaces of the coils were cylindrical.

The ratio of the greatest displacements was ·643. It may be conceded, therefore, that the questions of magnetization and placing received a fairly complete answer. In the final arrangement of the galvanometer with horseshoe magnets, like those described by the Messrs. Gray, the lengths of the yokes of the magnets was 8·5 centim., and thus this was the distance between the centres of the coils. The legs of the magnets were 3·8 centim. long, and they were magnetized like the trial magnet B. When in position, the magnet-legs projected into the coils to an extent of about $\frac{1}{8}$ the axial dimension of the latter. The suspension was two washed silk fibres (one would not carry the load) 16 centim. long.

The test for sensitiveness was made by running a large Clark cell (already described) through 10,000 legal ohms, and a certain small resistance taken out of an ordinary Bridge-box. The terminals of the Bridge-box were coupled up through the

galvanometer to a megohm. The accuracy of this method of testing has been already established.

The distance from the galvanometer-mirror to the scale was 155 centimetres ; and the light-spot was very good, showing the wire image as sharply as the lines on the scale. The scale itself was divided into millimetres. In the final test the period of vibration of the magnet system was 80 seconds, and the resistance out of the Bridge-box was 100 ohms. The E.M.F. acting through the megohm and galvanometer and

100 ohms was therefore $\frac{100}{10100}$ (Clark cells, say .0145 volt.

The current was therefore $\frac{.0145}{116000} = 1.26 \times 10^{-7}$ amperes.

Employing the method of vibrations and neglecting the correction for the extremely small log. decrement, the battery being of course reversed and several experiments made, it was found that the double deflexion amounted to 5 scale-divisions (millims.). Hence the deflexion corresponding to 1.26×10^{-7} amperes is 2.5 divisions. Now I do not think that, bearing in mind the lengthy period of the system, it would be possible to read to more than .5 division. The difficulty comes in in eliminating air-currents ; as far as the scale went I could read certainly .2 division, so there is no advantage to be gained in having the scale further, or even so far away. We may therefore say that five times the least measurable deflexion is given by 1.26×10^{-7} amperes, or that the least measurable deflexion itself is given by 2.5×10^{-8} amperes. This, I may mention, is only to be obtained when the magnets are judiciously kept from swinging by an extra controlling magnet, worked carefully from a distance till the vibrations get small, so as to jam against the side of the coil. Some fine quartz threads were also prepared by Boys' method, but no appreciable improvement introduced by their use could be detected. This may, however, have been on account of their thickness ; for they could be seen, with a little practice, in a good light, and when laid on a bit of black silk. I therefore came to the conclusion that neither I nor my instrument-maker (who is fairly good) could hope to compete in such delicate work with the Messrs. Gray, who, using this type of galvanometer, attained a sensitiveness of one half millimetre-division with a current of about 10^{-11} amperes, and that with a scale fairly close to the instrument, and with a manageable period of vibration.

In consequence of this failure I determined to use the four coils constructed for the Gray galvanometer, for an expe-

riment in which they were employed in the usual manner. They were therefore roughly mounted, and provided with an astatic combination of the kind previously described. The results were so encouraging that the same methods were pursued further. In the final form the suspension was a quartz fibre, 85 centim. long, suspended in a carefully chosen glass tube. With such long suspensions the tube must be very straight, and the arrangement for raising and lowering the suspended parts must be very good. After trying several arrangements for this, I adopted a pointed piston working into the tube and passing through a stuffing-box. The coils were of course anything but suited, as far as shape goes, for their present arrangement; however, they did what was requisite, though I have no doubt that coils might be made to increase the sensitiveness tenfold.

The mirror was in this case suspended midway between the magnets, and, for want of a better, was so thin that it got a little pulled out of shape by the paint which was used to fasten it to the aluminium wire. This fortunately turned out to be an advantage in some respects, for the vertical wire and the paint on the back of the mirror, by a happy accident, made the figure of the mirror practically that of a portion of a cylinder with a vertical axis; consequently, using a very good lens of 40 inches' focus, a good image of the light-spot was obtained at a distance of three metres. It became evident at once that the two real difficulties in securing sensitiveness lay in preventing air-currents and in adjusting the controlling magnet.

The first was finally attained by making the instrument practically air-tight; and, by means of a diaphragm, stopping down the beam of light to very nearly the size of the mirror—in this case of about 1 centimetre diameter. During the measurement of the resistance of an impure sample of sulphur this protection against air-currents was found to be insufficient, and the galvanometer was further protected by enclosing it in a cardboard box. If it ever becomes necessary to make an instrument to be sensitive to, say, 10^{-13} amperes, I shall have the support for the controlling magnet absolutely independent of the galvanometer-case, so that any vibrations set up in adjusting the magnet shall be transmitted only through heavy masonry. In the galvanometer now being described the controlling magnet could be raised by a nut and screw combination, itself sliding on a brass tube attached to the case of the instrument. The glass tube containing the fibre was clamped at its upper end to a very heavy stand of brass and lead, and this practically sufficed when the sensitiveness got to be of the order of one division to 10^{-11} amperes.

The arrangement for adjusting the distance of the controlling magnet was arrived at after several trials. It consisted of an apparatus sliding and clamping on the brass tube, with a nut and screw for fine adjustment. It was intended to use a worm-wheel and screw for the adjustment in a horizontal plane; but this was found after a little practice to be unnecessary, although it would be convenient.

The details of this arrangement for raising and lowering the magnets will be understood from the drawing of the galvanometer for medical purposes on Pl. XIV. figs. 1 and 2, see p. 416, *suprà*. Many experiments were made in order to decide the relative merits of quartz and silk fibres. At first it was thought that silk did as well; but after a time a great deal of trouble with the zero was traced to the silk, and attempts were made to use finer quartz threads. In this, owing to the skill acquired by Mr. Pollock, I was finally successful. As I have had about a year's experience in drawing quartz threads, I add the following notes on the process in hope that they may prove of use to others. The difficulty is to get a large enough bit of quartz fused onto a suitable handle. The best way of managing this is first to heat a bit of rock crystal red-hot in an ordinary crucible and keep it heated for about an hour. On cooling, it will be found to have split into fragments of all sizes; one of these is chosen, and supported on a bit of lime or on a massive bit of iron, and is then fused under the oxyhydrogen (not oxy-coal gas) jet. When it has once been got glass-like it never cracks again, no matter how suddenly it may be heated. Porcellaneous quartz draws into rotten threads, as might be expected. Two bits of fused quartz having been prepared they may be fused to the ends of two bits of clay tobacco-pipe, and can then be manipulated in the oxyhydrogen flame without trouble. There is no difficulty (when once the short thick threads have been drawn by hand) in the subsequent shooting. I most cordially indorse all that Mr. Boys says in favour of this admirable invention.

Another difficulty lies in the obtaining of a reasonable degree of astaticism. It has already been shown that it is practically possible to increase the astaticism of a magnetic combination by careful methods of magnetization and manufacture of the magnets: but the astaticism thus in general obtained is by no means perfect. The investigation of this matter was undertaken by Mr. Adair, and proved to be difficult and unsatisfactory. In the first place it was necessary to determine the coefficient of torsion of the silk fibre to be used in the experiments. This fibre was about thirty inches long, and before mounting had been boiled in a tube of water. A copper disk, made up with a mirror so as to have about the

same weight as nearly as possible as the astatic combination to be examined, was suspended from the fibre in a vibration-box furnished with a long glass tube. The copper ultimately employed was supposed to be electrolytically pure. The combination at first was slightly diamagnetic, but became much less so as the paint-cement dried, and was finally almost indifferent to any means we could find for testing it. From experiments with this disk, whose weight and moment of inertia were known, the coefficient of torsion of the fibre was found to be $T = \cdot 000115$ C.G.S., with a load of $\cdot 287$ gram.

Two astatic combinations were next mounted and tested. The first was the one that had already done some work in the galvanometer, the second was carefully made for the purpose. The moment of inertia of the first was found to be $\cdot 02126$, and of the second $\cdot 03274$.

With both combinations two sorts of experiments were made. The time of vibration of each was determined, and the change of zero produced by twisting the upper end of the fibre through a known angle, generally 2π . From these well-known methods it was found that the systems had a period of vibration of about $2\cdot 51$ seconds only. This corresponded to a value for the moments of the forces of about $\cdot 1270$. The magnitude of this number, as well as the positions taken up by the combinations, showed not only that the astaticism was far from perfect, but also indicated the cause of this: the magnets were not really in one plane. Tentative twisting of the aluminium wires was then resorted to, with the result of bringing up the periods of vibration to 11 and $12\cdot 8$ seconds respectively. In this latter case the moment was reduced to $\cdot 0064$. The needle that had been brought to a free period of 11 seconds was mounted in the galvanometer, and by means of the controlling magnet was brought to have a period of 36 seconds, corresponding to a magnetic moment of about $\cdot 0007$ C.G.S. During the experiments on resistance the period was got considerably longer than 35 seconds.

The next paper, on the Resistance of Impure Sulphur, contains the details of the method employed to find both the specific resistance of the gums mentioned and of sulphur. As no useful result is to be expected from a publication of the long series of experimental numbers obtained in the work on Gums, I refer to the following paper for the description of the *method* employed, since it remained constant throughout.

LV. *On Measurements of the Resistance of Imperfectly Purified Sulphur.* By Prof. RICHARD THRELFALL, and ARTHUR POLLOCK, Esq.*

THE galvanometer having been brought to a state of sensitiveness of 5 scale-divisions for 10^{-11} amperes, the measurement of the resistance of the sample of sulphur in question became a tolerably easy matter. The sulphur had been supplied by Messrs. Hopkin and Williams as "precipitated, washed." It looked clean when melted; but on examination turned out to have the following substances existing as impurities:—

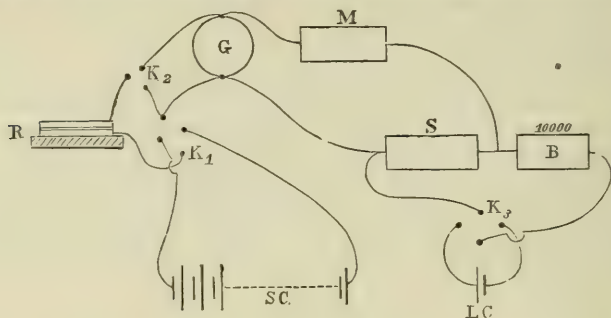
Calcium sulphate.
 Ferric oxide.
 Organic matter.

Dr. Helms, Demonstrator of Chemistry in the University of Sydney, was kind enough to investigate a sample of this sulphur with a view to discovering whether it contained selenium or tellurium. The result of his examination of about two hundred grammes of the substance was, that neither of these substances was present in quantity large enough to be detected. The examination was carried out by means of the oxidation and sulphurous acid method; and also by the cyanide method. We desire to express our thanks to Dr. Helms. The importance of this result, so far as our work is concerned, lies in the fact that it shows that pure sulphur can probably be obtained from the sample at hand by means of distillation. The following measurements refer to the unpurified sample; the only substance existing in any considerable quantity was calcium sulphate. The sample was probably rather more pure than ordinary "roll" or flour sulphur. The general arrangement of the apparatus will be clear from the following diagram (p. 470).

By means of the key K_1 a current can be sent in either direction through the resistance R and the galvanometer G . The source of this current is a suitable number of small Clark cells SC . The E.M.F.'s of these cells and of the large one LC were watched during the experiments with the aid of an auxiliary galvanometer and balance arrangement not shown in the diagram. The key K_2 allowed the upper metal

* Communicated by the Physical Society: read March 23, 1889.

plate of the resistance arrangement to be put in communication with the last cell so as to charge up without allowing the



current to rush through the galvanometer. After charging, the connexions were altered so as to allow the current due to the leak through the sulphur to pass through the galvanometer. The E.M.F. of the small cells was not changed by this amount of leaking, and the fall of the large cell was measured at the time in the usual manner. The experiment consisted in taking plugs out of the box S till the galvanometer gave the same deflexion whether the current was sent through it by the number of cells through the sulphur, or by the adjustable fraction of the E.M.F. of the large cell through a megohm. The sum of the resistances of S and R was always 10,000 legal ohms. The arrangement of the observations was such that they interlocked in time.

The absolute value of the result, therefore, depends chiefly on the value of the megohm. Respecting this standard I wrote to Messrs. Elliott to inquire what degree of reliance could be placed on it, and was informed in reply that it had been tested against a standard in blocks of 100,000 ohms each, and was right within the variation produced by one degree of temperature. The wire was of German-silver. As the present results are interesting only with respect to the method and the resistances of the sample under varying E.M.F.'s, the absolute value of the megohm is of comparatively small importance.

The following table gives the data of the experiments on the resistance of the sulphur:—

Date. 1888.	Temperature of room.	No. of small cells used.	E.M.F. of set of small cells.	E.M.F. of large cell short-cir- cued through 10,000 L. ohms.*
Oct. 20 ...	17.0 C.	20	volts. 28.661	volts. 1.432
20 ...	17.0 C.	40	57.312	1.432
21 ...	16.2 C.	40	57.312	1.432
21 ...	16.2 C.	20	28.661	1.432

Date. 1888.	Resistance taken out of S.	Thickness of layer of sulphur.	Area of layer of sulphur.	Resulting specific resist- ance of the sulphur.
Oct. 20 ...	L. ohms. 9.0	centim. 0.05	sq. centim. 189.8	8.575×10^{13} l. ohms.
20 ...	15.0	0.05	189.8	10.286×10^{13} „
21 ...	10.7	0.05	189.8	13.307×10^{13} „
21 ...	5.8	0.05	189.8	14.425×10^{13} „

* Lord Rayleigh has kindly pointed out to us that by a mistake in our method of reduction we have slightly underestimated this value. The result will be that the absolute value of the resistances as given are very slightly too large.

The following is a sample of the readings taken :—
Deflexion of Galvanometer with current from 20 Clark Cells
sent through the Sulphur.

Deflexion.		Double deflexion.	Zero from deflexion.	Observed zero.
Right.	Left.			
Divs. of scale.	Divisions.	Divisions.	Divisions.	Divisions.
+60	+145	85	+102	+95
+50	+145	95	+97	+100
+60	+148	88	+104	+100
+70	+150	80	+110	+100
+65	+143	78	+104	+100
+75	+150	75	+112	+98
+68	+143	75	+107	+98
+70	+135	65	+102	+90
+40	+135	95	+87	+88
+60	+140	80	+100	+85
+45	+133	88	+89	+85
+35	+115	80	+75	+80

Deflexion of Galvanometer with fraction of current from large Clark Cell.

Resistance taken out of S.	Deflexion.		Double deflexion.	Zero from deflexion.	Observed zero.
	Right.	Left.			
L. ohms.	Divisions of scale.	Divisions of scale.	Divisions of scale.	Divisions of scale.	Divisions of scale.
10	+55	+150	95	+103	+105
9	+60	+143	83	+101	+103
9	+60	+145	85	+101	+103
8	+68	+140	72	+104	+103
8	+70	+140	70	+105	+103
9	+60	+145	85	+101	+100
8	+63	+130	67	+96	+90
9	+50	+133	83	+91	+90
9	+45	+130	95	+92	+85
9	+35	+120	85	+77	+80
9	+35	+120	85	+77	+80
8	+40	+115	75	+77	+78
8	+40	+115	75	+77	+80

Rejecting those observations with the sulphur in which the zero from the deflexions differs from the observed zero by 10 divisions and over, the mean double deflexion with a current from 20 Clark cells sent through the sulphur is 85·5 divisions, and the mean deflexion with the current from the large Clark cell when $S=9$ ohms is 85·9 divisions.

The difficulties which had to be met during these measurements seemed to arise from what, following Mr. Bosanquet, we at first called "ghosts." These phantoms, however, seem to have arisen from people opening and shutting doors with iron locks; as a general rule the galvanometer was got properly sensitive overnight, and it was found that if on the following morning the light-spot was where it had been left, then the observations were practically successful; if, on the other hand, the light-spot had gone off the scale, then there was not much use in going on. The disturbance of the fibre consequent on restoring the old zero did not seem to wear itself out under at least twelve hours. It is practically impossible to get a galvanometer of this degree of sensitiveness to work with a silk fibre, the zero being always on the move. In order to get rid of air-currents the ventilators of the room required to be covered up, and the well-made galvanometer-case had to be enclosed in a cardboard box. The insulation at first gave great trouble. It is necessary to support the

wires on insulating stands of the paraffin-bottle form, the cells, resistance-boxes, galvanometer, &c. on sheets of glass, themselves resting on small cylinders of paraffin. The insulation of the handles of the keys requires attention. Paraffin keys are much better than ebonite ones.

In pushing the sensitiveness of the galvanometer beyond this point, the following precautions should be observed besides those already mentioned. The arrangement for supporting the magnets should be quite independent of the arrangement for supporting the suspension. The base of the instrument should be of gun-metal and all the framework of metal. The adjustments of the controlling magnets must be capable of being made with extraordinary accuracy. The mirror must be good enough to be used in a telescope. The quartz fibre should be at least six feet long; it must be cemented to its connexions with hard paraffin. The whole apparatus should, we think, be placed in a thick soft iron cylinder, but about this we are not sure. We are tolerably certain, however, that it is in anybody's power to construct a galvanometer on these lines with a sensitiveness of 10^{-13} amperes per scale-division and a time of swing of about 40 seconds. Such an instrument, however, could only be used in a tolerably non-magnetic building, and one steady enough to be free from the vibration caused by people walking about. Our best results were got at night and on Sundays, and this in spite of the room having a concrete floor reposing on twenty feet of broken stone and all the instruments being supported on slate benches.

As to the results quoted, no discussion will be given here as we are investigating pure samples of sulphur. It may, however, be mentioned that the resistance depends considerably on the time the current has been flowing, on the electromotive force, and on the temperature.

LVI. *On the Relation between Fluorescence and Arrangement of Molecules.* By B. WALTER*.

IT is well known that the intensity of the fluorescent light from solutions of many fluorescing materials at first increases with the dilution and afterwards decreases again. In order to understand this phenomenon rightly one must compare, for different degrees of concentration, as Stokes

* Communicated by the Author, and translated from the MS. by James L. Howard, D.Sc.

has already done, the ratios of the amounts of light emitted by a given quantity of fluorescent substance to that absorbed by it; since it is obviously upon these that the fluorescibility depends. From theoretical considerations Stokes* showed that in such solutions this quantity first increases with the dilution and then finally becomes constant. Lommel, on the contrary, in one of his numerous works on the theory of fluorescence, has maintained that the fluorescibility, his factor α , continually increases with increasing dilution†. A decisive answer to this question can only be obtained by actual measurement of the quantities involved in it.

My first observations of this kind, which I began in the winter 1887–8, and in which, for want of sunlight, the multi-coloured light of a petroleum lamp served to excite the fluorescence, could yield no decisive result on account of the theoretical difficulty in making a comparative estimate of the separate energies of fluorescence of different wave-lengths‡. On account of this I determined to repeat the experiments with homogeneous sunlight, for which the bright spring of 1888 offered a favourable opportunity. Experiment now decided undeniably in favour of Stokes. At the same time, on considering the special results of my measurements in connexion with some phenomena previously only slightly noticed, the cause of that remarkable behaviour of fluorescing solutions, which Stokes has not explained, became evident.

The following notice is an abstract of my complete paper §:—

I. *Measurements of the Fluorescibility.*

As fluorescing substance ammonium fluoresceïn (more shortly fluoresceïn) was taken in aqueous solution; and 23 different solutions of it were experimented upon, whose degrees of concentration varied from 0.000001 to 40 per cent. of salt. The exciting pencil of monochromatic sunlight was obtained by throwing a spectrum on the screen AB by means of a slit S_1 , the prism P_1 , and the lens L_1 ; then by means of a second slit S_2 in the screen AB the desired rays could be sifted out and rendered parallel again by a cylindrical lens L_2 . But before this pencil of rays fell upon the solution of fluoresceïn under investigation, and contained in the cell G_2 , it had to pass through a fairly dilute solution of the same

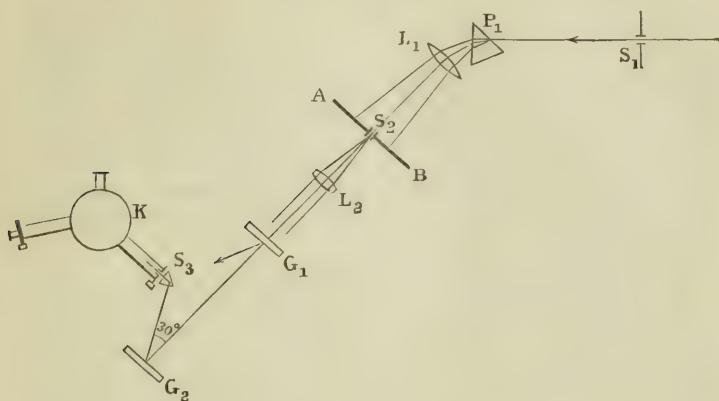
* Stokes, Phil. Trans. 1852, p. 535.

† Lommel, Pogg. Ann. clx. p. 76 (1877).

‡ Walter, Wied. Ann. xxxiv. p. 316 (1888).

§ Walter, Wied. Ann. xxxvi. p. 502 et seq. (1889).

salt in the vessel G_1 , and the fluorescent light from the latter served as the standard with which the intensities of light from the 23 solutions mentioned above were compared.



With the fluorescent light emitted from G_1 and G_2 in all directions there was mingled a comparatively large quantity of light from the exciting pencil, scattered at the sides of the vessels G_1 and G_2 ; this had necessarily to be separated from the fluorescent light itself. As this was only possible by a spectroscopic method, the photometer chosen was that of Vierordt (K), which is nothing more than a spectroscope with two slits, one immediately above the other, at S_3 . In this case the slits were each covered with a totally reflecting prism, so that one of them received light from the left, the other from the right. In the figure only one of these prisms can be shown. These prisms received the fluorescent light from the two vessels G_1 and G_2 respectively. After the widths of the slits had been adjusted until they gave, on looking through the eyepiece, spectra of the fluorescent light of equal brilliancy, the ratio of these widths, which were measured by micrometer-screws, gave directly the intensity of light from the substance under examination in the vessel G_2 .

The "fluorescibility," however, depends not only on the intensity of the fluorescent light, but also on the quantity of light absorbed; and, moreover, it is evident that in the latter term we must take into account the absorption of the fluorescent light itself. Now there is a simple theory, for which my original paper must be referred to, according to which the fluorescibility f , as given by the experiment, is

obtained with sufficient accuracy by the formula

$$f = \frac{F}{1 - a\alpha^{\sec. 30^\circ}},$$

in which F is the intensity of the fluorescent light from G_2 , measured as stated above; and the fractions a and α denote the "coefficients of transmission" (Langley) of the fluorescent light emerging from G_1 for the film of liquid in G_2 . They therefore express in what ratio the light incident on G_2 is weakened on emergence again. These coefficients were likewise determined by the Vierordt spectrophotometer, and thus all the data requisite for the calculation of the fluorescibility were obtained.

The following results were obtained:—

(1) The fluorescibility of very concentrated solutions of fluoresceïn (from 40 per cent. down to about 3 per cent. of salt) is zero, or at any rate very small; from this point it suddenly increases very quickly as the solution is rendered more dilute, the rate of increase being at first fairly uniform. But from 5 per cent. downwards the increase becomes more and more slow, and it ceases entirely when the solution contains 0.02 per cent. of salt; so that the fluorescibility remains constant from this point onwards to the most dilute solutions, the observations extending as far as a 0.000001 per cent. solution.

(2) The alterations of the fluorescibility remain the same whatever be the wave-length of the rays used to excite it; in the experiments rays were used both below and above those giving the maximum of absorption.

Alcoholic solutions of Magdala red of different degrees of concentration showed the same fluctuations of fluorescibility, except that in this case no solution could be obtained so concentrated that the fluorescibility became absolutely zero.

II. Theoretical Deductions.

The fact that the fluorescibility remains constant in the large number of solutions having a greater dilution than 0.02 per cent. must be regarded as the most important of the results just stated; for this means that under these circumstances the same quantity of substance always gives out the same quantity of fluorescent light. This can lead to no other theoretical conclusion than that the particles of fluoresceïn, which give rise to the light, preserve the same constitution unaltered throughout the whole of the range of dilution considered; and this conclusion agrees very well with the fact

that the absorbing power of fluoresceïn also showed itself constant throughout this range, whereas in stronger solutions this was no longer the case. What is the cause then of the irregularities in the fluorescibility and absorption in these latter solutions? The second of the results stated above points to the explanation. For since, according to it, the fluorescibility alters in the same manner for all rays producing the fluorescence, the decrease in this quantity in concentrated solutions could hardly be produced by any mere weakening of these waves (such as, for example, might be explained by the fluoresceïn molecules being too crowded), for then different wave-lengths would necessarily produce some difference in the effect; the explanation is rather to be sought in the decrease in the number of those molecules by which the fluorescence is set up. Fluoresceïn and similar compounds must according to this view exist in solutions of different degrees of concentration in at least two molecular conditions, a fluorescing and a non-fluorescing one; and indeed this hypothesis was soon placed beyond a doubt by the discovery of a very remarkable phenomenon of fluorescence.

Before I describe this phenomenon I will, for the sake of ease in referring to them, distinguish three grades of solution of the bodies under consideration:—

(1) Those in which only non-fluorescent groups of molecules exist (Group solutions).

(2) Those in which these are gradually disintegrated and pass into smaller fluorescing molecules (Transition solutions).

(3) Those in which this transition is completed (Perfect solutions).

In perfect solutions we have, according to the above, to deal only with molecules all having the same properties, which for shortness I shall call "single" molecules, and for which this law holds good:—Every single molecule throughout the whole range of dilution in which it preserves its single condition absorbs always the same fraction of the quantity of light falling upon it, and converts always the same fraction of the absorbed energy into fluorescent light. But as soon as the single molecules begin to arrange themselves in groups, as is the case in transition solutions, the absorption becomes quite irregular, and in the group the property of fluorescence is entirely lost.

These and all statements in the latter part of this paper are based upon the following phenomena. If a spectrum was allowed to fall on a perfect solution of fluoresceïn or Magdala red, one saw that each ray absorbed by it gave rise to a corresponding quantity of fluorescent light; in transition

solutions, on the contrary, a number of colours disappeared by absorption in the liquid which gave no such light; and indeed the production of fluorescence took place over exactly the same range of wave-lengths as in the most concentrated of the perfect solutions, while the absorption extended much further.

It follows clearly that in transition solutions also it is only the single molecules which produce the fluorescence, and that the portion of light absorbed by these solutions which does not reappear as fluorescent light is taken up by the groups of molecules. The fluoreescibility of a transition solution must therefore decrease with increasing concentration for three reasons:—(1) because the number of single fluorescing molecules is constantly getting reduced; (2) because the groups of molecules take up an ever-increasing proportion of that light which can produce fluorescence; and (3) because the groups of molecules exert a constantly increasing absorbent action upon the fluorescent light which is formed by the single molecules still remaining. This can be studied extremely well by observing the shrinking up of the band of light at the less-refrangible end in the case of the spectrum of fluorescent light, so that this alone affords a means of distinguishing at a single glance a perfect from a transition solution.

Stated more exactly, the three laws deducible from the above phenomena are the following:—

(1) In transition solutions there are present at the same time both groups of molecules and single molecules; and as the dilution increases the latter multiply at the expense of the former.

(2) Only single molecules can give rise to fluorescent light, not groups of molecules.

(3) The range of absorption by single molecules extends between quite definite wave-lengths; groups of molecules on the other hand absorb as a rule the neighbouring parts of the spectrum as well.

The latter phenomenon was most striking in the case of fluoresceïn and Magdala red; for while the absorption spectrum of a very thick layer of a perfect solution ended quite suddenly in the red with a sharp boundary, the absorption of a transition solution, even when the absorbing layer contained altogether less material, stretched beyond this boundary and was gradually lost on the other side of it (between it and the ultra-red). In the case of eosine no such difference was found, so that in this compound the group absorbs hardly any more rays than the single molecule.

These ideas were supported by another series of phenomena.

The fluorescent light of transition solutions of fluoresceïn and cosine increases in intensity considerably as their temperature is raised, doubtless because warm water decomposes the groups of molecules of these substances more easily than cold water. In Magdala red the contrast is still greater; cold water cannot decompose its groups of molecules at all, but warm water effects the decomposition fairly easily. That these phenomena cannot be ascribed to an increased freedom of vibration, produced by the application of heat, is proved by the alcoholic transition solutions of Magdala red, since in them the fluorescibility decreased slightly on heating. According to what has been said above this was rather a proof that warm alcohol does not dissolve Magdala red so easily as cold; and indeed a cold saturated solution of it became turbid on heating, an indication that the solid substance was being deposited again.

A further circumstance which pointed to the existence in transition solutions containing ammonium fluoresceïn of a more complicated molecular arrangement than in perfect solutions, was found in the fact that the former gave with mineral acids an immediate dense precipitate; the latter, on the other hand, remained perfectly clear, and only after several hours deposited fine crystalline needles of fluoresceïn itself. Although it is generally not uncommon for a stronger solution to yield a precipitate more quickly than a weaker one under similar circumstances, yet the contrast is here so great and the accompanying phenomena so remarkable that one cannot refrain from bringing it into connexion with what has already been stated with considerable certainty concerning the differences in grouping of molecules.

Finally, concerning the non-fluorescent group-solutions of fluoresceïn, it was noted that they all possessed a surface colour, which became stronger with increasing concentration but whose quality remained exactly the same; even the solid body itself—of course not commercial acid fluoresceïn, but ammonium fluoresceïn—possessed the same surface colour. It is seen from this that fluoresceïn in its group-solutions must still possess a stationary molecular condition, which must be somewhat like that of the solid body. This theory receives great support from the circumstance, proved in a later contribution*, that the index of refraction of these solutions increases in the same ratio as the percentage composition.

I cannot close this abstract without mentioning one obser-

* Walter, Wied. *Ann.* xxxviii. p. 117 (1889).

vation which has no direct connexion with the above, but which may probably be of immense importance in the theory of fluorescence, namely that fluorescein and its ammonium salt, although they have quite different absorption spectra, yet give out qualitatively exactly the same kind of fluorescent light. We have then here, so to speak, two dissonant strings of different materials; and there is placed before us with considerable emphasis this remarkable peculiarity of fluorescence as opposed to acoustic resonance, that the wave-length of the light exciting the sympathetic vibrations is, within certain fixed limits, of such slight importance.

Hamburg, Oct. 1889.

LVII. *On a Physical Basis for the Theory of Errors.*

By CHARLES V. BURTON, D.Sc.*

1. **I**N deducing a law of error, two courses seem open to us. We may make our assumptions as general as possible, so that our results shall have the widest application, and shall in the long run approach most nearly to the truth; or we may treat each separate case as a special problem in probability, taking account of all that we know concerning the actual conditions.

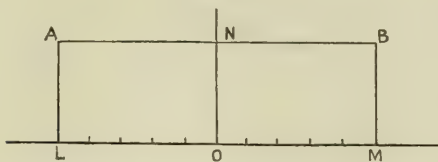
I shall here endeavour to illustrate the latter method by means of some examples; proceeding next to the resultant law of error when two or more elements are combined which are independently subject to error. The most advantageous combination of fallible measures will then be shortly discussed, and, finally, subjective or personal errors will be considered.

2. Suppose that we are given a series of numbers, known correctly to any required number of places, and that from this we write down the same series correct to four places. There will be no uncertainty in the operation unless the digit in the 5th place is 5, and all the remaining digits zero; and (in general) the chance of this occurring is indefinitely small. The limits of possible error are obviously $\pm .00005$, and all errors between these limits are equally probable, unless from our knowledge of the series we have *à priori* evidence to the contrary. The curve of error (as one may call it) is thus a finite straight line AB (fig. 1), parallel to the axis of errors LM, and bisected by the ordinate of no error, ON. If the original table is carried only to (say) 5 places the case will be somewhat changed. About $\frac{1}{10}$ of the series of numbers will have 5 in the 5th place of decimals; the remaining $\frac{9}{10}$

* Communicated by the Physical Society: read November 1, 1889.

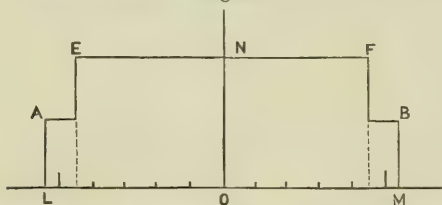
of the series will all be *correct* to 4 places, and will have errors ranging uniformly between ± 0.00045 . Of the first-

Fig. 1.



named $\frac{1}{10}$ of the series, half will have errors between $+0.00045$ and $+0.00055$, and half will have errors between -0.00045 and -0.00055 ; the distribution between these limits being uniform*. The corresponding curve of error is given in fig. 2.

Fig. 2.



3. A similar case is the following. Suppose we have to record successive positions of an index upon a fixed scale, which is graduated in centimetres, and that readings are to be taken to the nearest centimetre. If our judgment were infinitely acute, the errors would lie uniformly between ± 0.5 centim.; but in practice there will also be subjective errors, the consideration of which is left to a later section (§ 10).

4. Next let us consider the error introduced by friction into the equilibrium position of a movable index. Suppose that the index has one degree of freedom, and that if friction were removed its vibrations would be simple harmonic; the frictional coefficient being the same at rest and at all speeds. During a half-swing—say to the right—there will be a constant force (or a constant moment) of friction urging the index to the left, and its motion during the half-swing will be harmonic and in the same half-period as if friction were absent, the only difference being that the mean position of the half-swing lies somewhat more to the left. In the return half-swing there will be the same half-period, the mean position being equally displaced to the right. The amplitude

* That is, supposing that 0.00005 is added in the case of half of these numbers, and subtracted in the case of the remaining half.

is thus decreased by the same amount at each half-swing, until finally a half-swing leaves the index between the limiting positions of friction, where it remains permanently at rest. If the initial displacement (D) of the index was large compared with the range of frictional error ($\pm d$), we may assign the same probability to all displacements between $D-2d$ and $D+2d$; and since the final displacement differs from D by an exact multiple of $2d$, it immediately follows that all final displacements between $\pm d$ are equally probable, larger errors being impossible. The curve of error will be like fig. 1.

If friction is greater when the index is at rest, the result is rather curious. Let $\pm d'$ be the limits of equilibrium under statical friction; then there will be equal probabilities of errors between the limits $2d-d'$ and $-d'$, and also between $-2d+d'$ and $+d'$. If d' is $< 2d$, these ranges of error overlap, and the curve of error is like fig. 3; if d' is $> 2d$ the curve is like fig. 4; if $d' = 2d$, we have simply fig. 5.

Fig. 3.

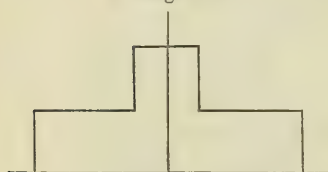
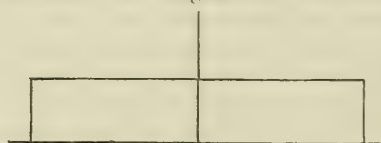


Fig. 4.



Fig. 5.



These results are easily obtained by considering initial displacements between the limits $4nd+d'$ and $4(n+1)d+d'$, where n is an integer; they refer of course to the actual, not to the observed position of the index. If, while the index was in motion, three successive excursions were read (with perfect accuracy), the inferred position of equilibrium would only be subject to an error due to *deviations* from the assumed laws of friction.

5. Now let a declination-needle which is to trace a continuous record be subject to frictional error. If the black line in fig. 6 represent the true declination-curve, the curve traced by the needle will be something like the dotted line. Here the law of error depends on the (variable) friction of the needle, and on the kind of changes which occur in the quantity

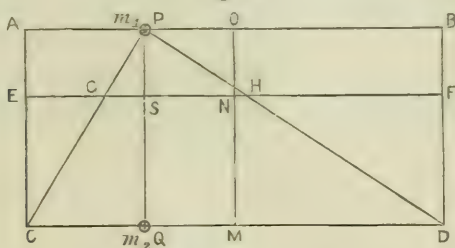
measured; it will further be influenced by the moment of inertia of the needle.

Fig. 6.



6. Enough has now been said to show that the law of error to be adopted depends in some measure on the nature of each special case; we may next consider how to find the law of error when two or more fallible elements are combined. To commence with, take two elements whose curves of error are of the type of fig. 1, the limits of error being $\pm m_1$ and $\pm m_2$ respectively. In the rectangle A C D B (fig. 7), let A O = O B

Fig. 7.



$=CM=MD$ = the unit of length; and let a particle be chosen whose mass is numerically equal to m_1 . If this particle be placed at OP to represent an actual error $=m_1OP$, it is evident that the *uniform* motion of the particle from A to B represents the distribution of errors between the limits m_1OA and m_1OB , that is, between $\pm m_1$. The second source of error may be similarly represented by a particle of mass m_2 which moves uniformly from C to D . When m_1 is at P and m_2 at Q , the resultant error will be $m_1OP + m_2MQ = (m_1+m_2)NS$, where S is the centre of mass of m_1 and m_2 .

To form the most general series of combinations, let m_2 move backwards and forwards very rapidly between C and D, always with (numerically) the same velocity, while at the same time m_1 moves uniformly and very slowly from A to B. By following the movements of the mass-centre along EF, we shall find the law of frequency of the resultant error. Join PC, PD, cutting EF in G and H; then while m_1 is passing through P, the mass-centre is moving uniformly between G and H; and as m_1 moves from A to B, GH moves

lowing the method of the previous section, let the mass m_1 move very rapidly backwards and forwards between A and U or between A and B, having at each point of its path a velocity inversely proportional to the corresponding ordinate; (adding U B to the path makes no difference, since the velocity in this part would be infinite). Similarly let m_2 move from C to D with a very small velocity, which follows a similar rule. First of all let the mass m_2 be passing through R ($NR = x_2$), so that the second error has the value $m_2 x_2$; also in fig. 9 let $MT = x_1$, $TT' = \delta x_1$, $OS = x'$, $SS' = \delta x'$. Then the chance of a resultant error between $(m_1 + m_2)x'$ and $(m_1 + m_2)(x' + \delta x') =$ the chance that m_1 is moving between T and T'

$$= y_1 \delta x_1 \div \text{area ALU}.$$

$$= y_1 \frac{m_1 + m_2}{m_2} \delta x' \div \text{area ALU}.$$

For simplicity, let the scale of ordinates be chosen so that the area $ALU = VKD =$ unity; then the chance that R may lie between x_2 and $x_2 + dx_2 = y_2 dx_2$.

Hence, taking both movements into account, the chance of an error between $(m_1 + m_2)x'$ and $(m_1 + m_2)(x' + dx')$

$$= \frac{m_1 + m_2}{m_2} dx' \int y_1 y_2 dx_2;$$

y_1 and y_2 being connected by the relation

$$m_1 x_1 + m_2 x_2 = (m_1 + m_2)x',$$

and the limits of integration being determined by

$$x_1^2 < 1, \quad x_2^2 < 1.$$

With similar notation for n independent sources of error, we shall have for the chance of a resultant error between $(m_1 + \dots + m_n)x'$ and $(m_1 + \dots + m_n)(x' + dx')$

$$dx' \frac{(m_1 + m_2)(m_1 + m_2 + m_3) \dots (m_1 + m_2 + \dots + m_n)}{m_2 m_3 \dots m_n} \\ \times \iint \dots \int y_1 y_2 y_3 \dots y_n dx_2 dx_3 \dots dx_n; \quad \dots \quad (2)$$

where $m_1 x_1 + m_2 x_2 + \dots + m_n x_n = (m_1 + m_2 + \dots + m_n)x'$, and the limits of integration are given by

$$x_1^2 < 1, \quad x_2^2 < 1, \dots x_n^2 < 1.$$

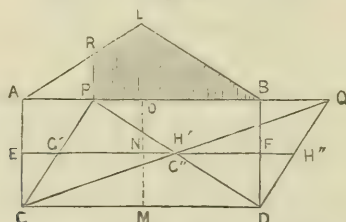
If we make n infinite, each of the y 's equal to $\frac{1}{2}$, and all the m 's equal, this reduces to the case from whose approximate solution Laplace deduced his law of error.

8. If we follow up the operation of § 6 by successively adding sources of error of the type of fig. 1, we shall find

that the resultant curve rapidly approaches the Laplacian form, the discontinuities being continually of a remoter order. Thus with two errors (fig. 8) the curve meets the axis of errors and has only discontinuities of direction; with three errors there are discontinuities of curvature, and so on. The curve produced by combining n errors of this type will have contacts of the $(n-2)$ th order with the axis of errors; and the resultant of two curves with contacts of the m th and n th orders respectively has contacts of the $(n+m+2)$ th order; for contacts of the m th and n th orders would be produced by combining $m+2$ and $n+2$ errors respectively.

To illustrate these principles, consider the error introduced into the indications of a balance by the friction of its knife-edges; and to simplify the case, suppose that the balance is allowed to come to rest before a reading is taken. We are only concerned here with errors in the *actual position* of the pointer, supposing the weights correct and the balance just. The friction at each knife-edge is a source of error, and the three sources of error are (at least very approximately) independent. Let the frictional coefficient at each knife-edge be constant, the maximum error due to the central knife-edge being double that due to either of the others; there are then three curves of errors of the type of fig. 1. The two equal sources of error being compounded will give the curve A L B (fig. 10), whose maximum error is equal to

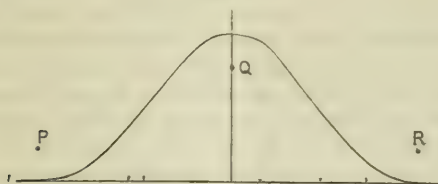
Fig. 10.



the maximum value of the remaining error. We shall thus have two equal masses, one moving rapidly backwards and forwards along C D with numerically constant velocity; the other moving slowly from A to B with a slowness proportional to the corresponding ordinate; and the expression (1) will evidently be applicable. Consider an element δx at H; then the second factor of (1) = chance that the slowly-moving mass lies between P and Q = the area P R L B. As H' moves from E to F, P Q moves from the position in which Q coincides with A, until P coincides with B. The resultant curve of error will consist of three arcs of equal parabolas (fig. 11) with foci at P, Q, and R. For deter-

minations by the method of double weighing, we must compound this curve with another equal and similar curve.

Fig. 11.



9. *Most advantageous combination.*—Suppose we have a number (n) of independent measurements ($a_1, a_2, \dots a_n$) of the same quantity. Let the chance of an error between x_1 and $x_1 + dx_1$ in a_1 be

$$\frac{1}{c_1 \sqrt{\pi}} \cdot e^{-\frac{x_1^2}{c_1^2}} dx_1, \dots \dots \dots (3)$$

with similar notation for the remaining measures—an assumption which is perfectly general. By adopting X as the true value we assume the errors in the respective measures to be

$$(a_1 - X), (a_2 - X), \dots (a_n - X).$$

If we know the limits of possible error for one of the measures, a_r , then $a_r - X$ must be made to fall within these limits. Subject to such restrictions we must make the product

$$\frac{1}{c_1 \sqrt{\pi}} \cdot e^{-\frac{(a_1 - X)^2}{c_1^2}} \frac{1}{c_2 \sqrt{\pi}} e^{-\frac{(a_2 - X)^2}{c_2^2}} \dots \frac{1}{c_n \sqrt{\pi}} \cdot e^{-\frac{(a_n - X)^2}{c_n^2}}$$

a maximum; that is we must make

$$\phi_1(a_1 - X) + \phi_2(a_2 - X) + \dots + \phi_n(a_n - X) \text{ a minimum.} \quad (4)$$

If Laplace's law of error applies to each of the independent measures, then

$$\phi_1(a_1 - X) = \frac{(a_1 - X)^2}{C_1^2}, \dots \phi_n(a_n - X) = \frac{(a_n - X)^2}{C_n^2};$$

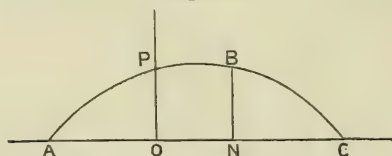
and (4) reduces to the method of least squares. We can also see from (4) how the most advantageous combination of measures depends on the laws of error in the separate observations; and that Laplace's law of error alone leads to the method of least squares.

As another particular case, let each of the quantities $\phi_1(x_1), \phi_2(x_2) \dots \phi_n(x_n)$ be constant between certain limits, and possible only between those limits; this will in general give a finite possible range to the value of X , all values within this range being equally probable.

The method of finding the most advantageous combination of measures by making the probability of the assumed value a maximum was given by James Ivory* ; it appears to me the most direct and satisfactory, and indeed the only method which is applicable in all cases. The intimate relation which has here been shown to exist between Laplace's law of error and the method of least squares is also in agreement with the results somewhat differently deduced in Ivory's paper.

10. *Subjective errors* remain to be considered. Suppose that in § 3, when the index is nearly halfway between two graduations, we may not decide correctly to which graduation it is nearest. Let A B C (fig. 12) represent the curve of

Fig. 12.



error when the observer attempts to place this particular index halfway between two graduations, O P being the ordinate of no error ; so that O C and O A represent the limits of error. Now if the distance of the index from a graduation lies between $+\cdot 5$ centim. $-O C$ and $-\cdot 5$ centim. $-O A$, its position will be taken as certainly nearer to that graduation than to any other, and the corresponding errors for such positions of the index will be uniformly distributed between these limits. Draw any ordinate B N of the curve A B C ; then the chance that in trying for the middle of a division we should *discard the position N for one more to the right*

$$= \text{area } N B C \div \text{area } A B C ; \quad (5)$$

when N is to the left of A, this chance becomes a certainty. The curve of error, then, for readings to the nearest centimetre will be straight and horizontal in the middle, the terminal portions being found by integrating A B C from A to C and from C to A.

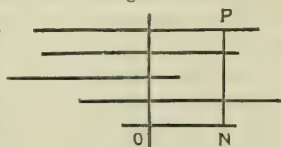
11. This method of treatment is not free from assumption ; it would only be strictly applicable if our judgment were on each occasion perfectly definite and decided, but at the same time subject to a variable error following a known law of frequency—like the definite but fallible indications of a physical index. But it is evident that when an index is sufficiently near to its true position (or to our more or less biased conception of its true position), we shall have no choice

* Phil. Mag. 1825, vol. lxxv. pp. 161 *et seq.*

in saying in which direction the error lies, and therefore no motive in altering the adjustment. Now the bias and want of acuteness of our judgment will of course be variable, the circumstances which affect them (experience, fatigue, illumination, &c.) being almost endless; but I think this principle may be conceded: *If an index be placed in a given position and examined from a given point of view, we shall judge the adjustment to be correct when, but not unless, the error falls between certain limits; such limits being determined by subjective and other conditions at the time of the observation.*

12. Now consider (merely for the sake of illustration) two ways in which an adjustment may be made. Suppose we are measuring the positions of spectrum-lines by bringing a wire into coincidence with them, and disregarding parallax and instrumental errors, let us confine our attention to errors of judgment in adjusting the cross-wire. First let the determinations be made in this (impracticable) manner: the micrometer head is first moved at random, and then the field observed, and if, in the judgment of the observer, the vertical wire coincides accurately with a spectrum-line, the micrometer reading is recorded. Whenever an apparent coincidence is registered, the error of judgment must have fallen between certain limits; and obviously it is equally likely to have any value between those limits. In a number of observations the law of error may be determined as follows:—Draw a number of horizontal lines (fig. 13) each representing on a convenient scale the limits of possible error at the time of an observation*. The relative frequency of an error ON will be proportional to the number of horizontal lines cut by the ordinate PN . In general the curve of error for a large number of observations will be straight and horizontal in the middle, falling down towards the ends so as to meet the axis of errors; if the limits of errors of judgment were invariable, the curve of error would be of the type of fig. 1.

Fig. 13.

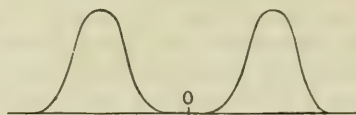


13. Perhaps in practice we shall more nearly approximate to the following plan:—Starting with the wire in a position which is evidently in error, we shall move it up to the spectrum-line until no further error can be detected. If we really stop moving the wire as soon as we cease to feel a distinct motive for doing so, we shall be just at the limit of possible error corresponding to that time. For a series of

* What we really require to know is the *law of frequency* for the limits of error.

single observations the curve of error would perhaps be something like fig. 14.

Fig. 14.



14. As a final example, suppose we are trying to place a small round body in a given position in a plane. Here the law of error will depend on our manner of testing the adjustment: whether we merely look up and down and from side to side to get two (very nearly independent) judgments of the corresponding coordinates, or whether we examine the object from every point of view to make sure that no error can be detected. The case may be illustrated mechanically by supposing two pendulums supported one on gimbals, the other after the manner of Foucault's pendulum; appropriate laws of friction being assumed. The law of error will further depend on whether we follow the method of § 12 or of § 13.

If we were trying to drive a billiard ball from one given position so as to make it come to rest in another given position (without rebounds), the errors of direction and of distance would, if small, be very nearly independent; the latter coordinate being probably subject to much greater variation.

We are led, then, to the following conclusions:—The law of error in a set of observations depends on the nature of each special case; and what may be called the probable law of error is determined by our knowledge of the conditions. The combination of three or more sources of error of comparable importance gives in general a law of frequency not seriously differing from that of Laplace; so that the method of least squares will be practically the most advantageous, except where a single source of error with a very different law is predominant above all the rest.

LVIII. *On a Method of Driving Tuning-Forks Electrically*
By W. G. GREGORY, M.A., Demonstrator in Physics at the
Royal Indian Engineering College, Coopers Hill*.

IN the ordinary method of driving tuning-forks by electricity, the battery circuit is completed just before the end of the stroke and broken again soon after the commencement of the return motion. Thus the fork receives its impulse at a

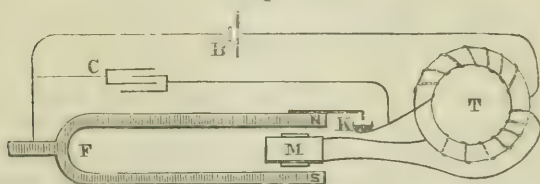
* Communicated by the Physical Society: read November 1, 1889.

most unfavourable moment, *i. e.* when it is stationary. The impulse can be considerably delayed by including in the battery circuit a solenoid into which a solid iron core is inserted more or less till the best result is obtained. This adjustment, however, varies with the amplitude of vibration and the alterations of the contact. The whole matter was discussed in a paper read before the Physical Society on June 26th, 1886, by Prof. S. P. Thompson*, who then suggested the employment of two similar forks which drove one another and settled down to a difference of phase of a quarter of a period. Each fork, therefore, received an impulse when moving at its greatest velocity in the middle of its stroke.

The method I now suggest accomplishes the same thing without the employment of a second fork, and moreover gives two impulses in each complete period instead of one.

The arrangement is shown in fig. 1.

Fig. 1.



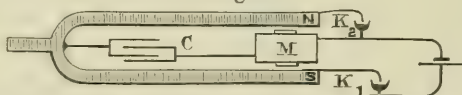
F is a tuning-fork mounted in the usual way, and provided with a driving electromagnet M, and contact-maker K. But instead of taking the main battery-current directly through the electromagnet M, it is sent through the primary coil of a kind of transformer T, the secondary circuit of which is connected to the electromagnet. Hence, at both make and break of the main circuit, momentary induced currents pass through the driving magnet alternately in opposite directions. By polarizing the fork so as to make it a horseshoe-magnet, the impulses are also made to act alternately as attractions and repulsions, and the fork receives two impulses for each complete vibration. By properly adjusting the contact when the fork is at rest, so as just to complete the circuit, the impulses will be given very approximately at the middle of the stroke, and are independent of the amplitude of vibration. The fork I tried made about 86 vibrations per second, and had prongs 20 cms. long and 1.8 cm. apart. The electromagnet M was formed by winding about 50 turns of no. 22 silk-covered copper wire round a core formed of a bundle of varnished iron wire. It was mounted on a wooden support capable of sliding between and parallel to the prongs. The transformer T consists of a core

* Proc. Phys. Soc. viii. pt. ii. ; Phil. Mag. [5] xxii. p. 216.

of cotton-covered iron bonnet-wire wound in the form of an anchor-ring having a mean diameter of 6 cms. and thickness 1 cm. Round this is wound the secondary coil of about 160 turns of no. 22 silk-covered copper wire, forming a single layer completely round, and outside this the primary of about 190 turns of similar wire. A condenser C of 4 microfarads capacity was connected across the mercury-break K to diminish the sparking. One small accumulator B sufficed to work the fork vigorously, the mean current consumed being about 2 amperes. By increasing the number of turns of wire, much less current would be required, but there would be a limit, since the lag must not be so great as to interfere with the proper timing of the impulses.

An alternative method, using far less current but requiring two contacts, is shown in fig. 2.

Fig. 2.



The contact K_1 charges the condenser C and K_2 discharges it. The currents of charge and discharge are both taken through the electromagnet M, and the action is the same as above, the fork being, of course, polarized. It would be necessary to wind the magnet M with many turns of wire, to use a battery of high E.M.F. and a large condenser. The latter, however, could probably be replaced by a polarizable battery of platinum or lead wires in dilute sulphuric acid. Except for economy of working, it is not so good a method as the first, as the adjustments of the contacts K_1 and K_2 must be very exact.

As regards magnetizing the fork, there can be no real objection to that, for in the usual method of driving it soon acquires a polarity. In fact the fork I first tried worked at once without any further magnetization.

LIX. *Proceedings of Learned Societies.*

GEOLOGICAL SOCIETY.

[Continued from p. 144.]

November 6, 1889.—W. T. Blanford, LL.D., F.R.S., President, in the Chair.

THE following communications were read:—

1. "Contributions to our Knowledge of the Dinosaurs of the Wealden and the Sauropterygians of the Purbeck and Oxford Clay." By R. Lydekker, Esq., B.A., F.G.S.

2. "Notes on a 'Dumb Fault' or 'Wash-out' found in the Pleasley and Teversall Collieries, Derbyshire." By J. C. B. Hendy, Esq.

The "Top Hard" Seam of the district is being worked in these collieries at a depth of 500 yards, where it has an average thickness of 5 feet, with a band of cannel in the middle. In the working it was found that the coal began to thicken, until it became double the usual size, the cannel also increasing in the "Top Seam," but in the Lower Seam running out altogether.

This double thickness of coal continued till the "wash out" was reached, when both coal and shaly roof disappeared, the space being replaced by sandstone similar to that of the beds overlying the shale. The clay floor of the Lower Seam had not been much interfered with, and this was followed for 60 yards, when the doubly thick seam was again met with, and on being followed gradually assumed its normal thickness.

No fossils have been noted in the "Wash-out" itself, the vertical extension of which is unknown.

3. "On some Palaeozoic Ostracoda from North America, Wales, and Ireland." By Prof. T. Rupert Jones, F.R.S., F.G.S.

LX. *Intelligence and Miscellaneous Articles.*

ON THE SECOND LAW OF THERMODYNAMICS.

BY PROF. EILHARD WIEDEMANN.

IN a paper on the Mechanics of Luminosity (*vide antea*, p. 152) I have minutely discussed the phenomena of luminescence; that is, of those processes in which the normal relation between translatory and intramolecular motion does not exist, and I have shown that these phenomena are far more widely diffused than is ordinarily assumed. In connexion with the introduction of the idea of the temperature of luminescence I made the following observation:—The assumption which is at the basis of the deduction of the second law of thermodynamics, that heat cannot pass without work from a body of lower to one of higher temperature, must, in accordance with the above considerations, be otherwise conceived, since when phenomena of luminescence occur such a transference may very well take place.

I take upon myself to communicate another conception of the principle of Clausius, which takes into account also the phenomena of luminescence.

Energy, corresponding to a definite time of vibration, always passes by radiation from one body to another, when in the first body the ratio between emission and absorption is greater for this kind of vibration than in the second case. Energy which corresponds to a definite intramolecular motion passes on contact or mixture from one body to another, if the ratio of the intramolecular energy, which, on the collision of the molecules, is changed into motion of translation, to the translatory motion which is converted into intramolecular motion is greater in the first body than in the second. A transference of energy takes place finally

if the energy of the motion of translation of the molecules of the first body itself is greater than that of the second.

From these determinations we get at a rigorous definition of the temperature of luminescence itself.

The temperature of luminescence for any kind of radiation or other intramolecular motion is that temperature to which a non-luminescent body must be brought, so that this motion may be equivalent to that of the luminescent body. It is clear that the second law of the mechanical theory of heat, which is based on Clausius' principle, cannot be applied in the form $\oint dQ/T=0$ in all the cases we have considered, but must be in accordance with the expression $\oint dQ/T < 0$.

I may be permitted to elucidate the first of the above principles by reference to some examples.

We bring calcspar from the temperature of 0° to phosphorescence; it is then bright red. Suppose we introduce it into a hollow platinum shell at a higher temperature, say 1° . Let the platinum sphere be supposed to be exhausted, so that there is neither convection current nor conduction. For the ultra-red rays which proceed from the platinum Clausius' law holds in its original form; but as calcspar is diathermanous for these rays, they do not heat it. We could finally put an envelope at 0° between platinum and calcspar, which should be adathermanous for the heat-rays and diathermanous for the visible rays. The red rays proceeding from the calcspar are absorbed by the platinum, and this is heated thereby; hence heat has passed from a cold to a warmer body without a simultaneous production of work.

In many flames we have to do with phenomena of luminescence; this follows in the first place from the experiments of W. v. Siemens*, and further from those of H. Ebert†, who proved that when a flame is dulled by introduction of carbonic acid, and is thereby greatly cooled, the emission in the ultra-violet at about 500° is almost exactly the same as that when the flame of 1000° is deadened by admission of air. If we suppose such a flame burning in a platinum cylinder at 100° , the emission of which is still small in the ultra-violet, and if we interpose a body which absorbs all the visible rays and only transmits the ultra-violet rays, then ultra-violet rays will pass from the flame to the platinum, so that they will be absorbed and the platinum heated.

If we do not restrict ourselves to the consideration of the transmission of energy from the luminescent body, but also take into consideration the source of energy which excites the luminescence, the principle of Clausius is thus valid in its old form, as is at once clear in many other cases. The luminescent body is then as it were only an intermediate member which effects the transition of a hot body to a cold one; an intermediate member which, however, is colder in the ordinary sense of the word than the two terminal members.

We have excited calcic carbonate by rays of the sun, which correspond to a much higher temperature than that which the platinum foil possesses. The transition takes place in such a manner

* Wied. Ann. xviii. p. 311 (1883).

† Sitzungsberichte der Phys.-med. Soc. Erlangen, July 1889.

that the energy proceeding from the sun is retained as vibrations in the molecules of the calcic carbonate.

I presuppose in this that in calcic carbonate we are dealing with photo-luminescence. If it were a chemical luminescence, we might take uranium nitrate or any other body as the basis of our considerations. In chemical luminescence the potential energy stored up in the impact of the molecules is transformed into kinetic energy, which also corresponds to a very high temperature.—Wiedemann's *Annalen*, xxxviii. p. 485, 1889.

ON THE MAXIMUM GALVANIC POLARIZATION OF PLATINUM

ELECTRODES IN SULPHURIC ACID. BY CARL FROMME.

In a former communication I made experiments on the determination of the maximum galvanic polarization of platinum electrodes in sulphuric acid of varying strengths. The electrodes had a surface either of 1 square centim. (large electrodes) or of 0.008 square centim. (small electrodes). If the kathode was small it always blackened, in consequence of the formation of platinum black, but if it was large no change in the colour of the surface was observed.

The following experiments on platinized platinum electrodes show the influence exerted by an initial strong platinization of the anode or the kathode on the maximum value of the galvanic polarization in sulphuric acid of varying strengths.

The following are the principal results of the author's researches:—

(1) A bright platinum foil when used as a kathode in dilute sulphuric acid becomes coated with platinum black, which with sulphuric acid of less than 3 per cent. can depress the polarization 0.9 volt. Strictly speaking, therefore, it is impossible to determine the maximum polarization in very dilute acid for a bright kathode.

(2) A copious electrolytic deposit of platinum black on the kathode from chloride of platinum does not diminish the polarization more strongly, so long as the concentration of the sulphuric acid is small. But in more concentrated acid (more than 3 per cent.) it is of greater action than the feeble spontaneous platinization.

(3) The decrease which the polarization experiences in consequence of covering the kathode with a thick layer of platinum black is greatest, as much as 0.9 volt, in the most dilute acids, and in the most concentrated (of 50 to 65 per cent.) it is only 0.1 volt.

(4) The platinization of the anodes, on the other hand, diminishes the polarization in the more concentrated acids more strongly than in the dilute, in such a way that while in the latter the polarization of bright electrodes decreases somewhat more in consequence of platinizing the kathode, in acid of about 60 per cent. the platinization of the anode has a very considerably greater influence (as much as seven times as great).

(5) The polarization of a voltmeter with bright electrodes varies with the concentration in a very complicated manner. Maxima and minima are often very near each other. High values near or even more than 3 volts are found with a small as well as with a great degree of concentration.

(6) But if the kathode is platinized the maxima and minima disappear almost entirely, and the polarization, which at first is only 2.1 volts, rises with increasing concentration pretty regularly to 2.8 to 2.9 volts, with a concentration of 65 per cent.

(7) If, on the other hand, the anode is platinized, the maxima and minima do not disappear in the strongly dilute acids, but the constant and small polarization remains in the concentrated acids (of 20 to 25 per cent.).

(8) The most regular course, and that which depends least on the concentration, and in accordance with 3 and 4 the smallest values of the polarization, is obtained by platinizing both electrodes.

(9) The high polarization of bright electrodes in comparison with the small one of platinized electrodes, if the acid is of small concentration, is therefore due in a great part to the polished condition of the kathode, and the occurrence of maxima and minima is due exclusively to this cause. On the other hand, the high values in the more concentrated acids are to be ascribed almost exclusively to the polished condition of the anode.

The explanation of these conditions results from the more ready formation of gas-bubbles on platinized as well as on polished electrodes, especially in the more dilute acids, as well as from the formation and accumulation of secondary products (persulphuric acid and hydrogen peroxide) at the bright anode in more concentrated acids.

(10) The simple relations of platinized electrodes and their small values of polarization are not manifested by bright electrodes even when their surface is materially increased.

(11) The polarization of bright electrodes varies somewhat with the preparation of the mixtures of sulphuric acid; that of platinized is, however, independent thereof.

(12) While half an hour to an hour frequently elapses before the maximum of polarization is attained with bright electrodes, platinized electrodes soon reach their maximum polarization.

In the more dilute acids it is the bright kathode, and in the more concentrated ones the bright anode which most slowly attains the maximum polarization. The greater the maximum, the farther in general is the polarization from it shortly after closing the current.

(13) The polarization increases in general with decreasing current strength, and to the greatest extent with bright electrodes. The effect of platinizing the kathode is that polarization in the more dilute acids is more independent of the strength of the current, while that of platinizing the anode is that polarization in the stronger acids is not so much affected by the strength of the current. The explanation of this readily follows from (9).

(14) The minimum resistance of the voltameter is at about that concentration at which the conductivity of the sulphuric acid attains its maximum.

(15) The polarization of platinum electrodes is at most only 0.1 volt higher than the smallest electromotive force which with bright platinum wires can disengage fresh gas under a pressure of one atmosphere.—Wiedemann's *Annalen*, xxxviii. p. 362, 1889.

INDEX to VOL. XXVIII.

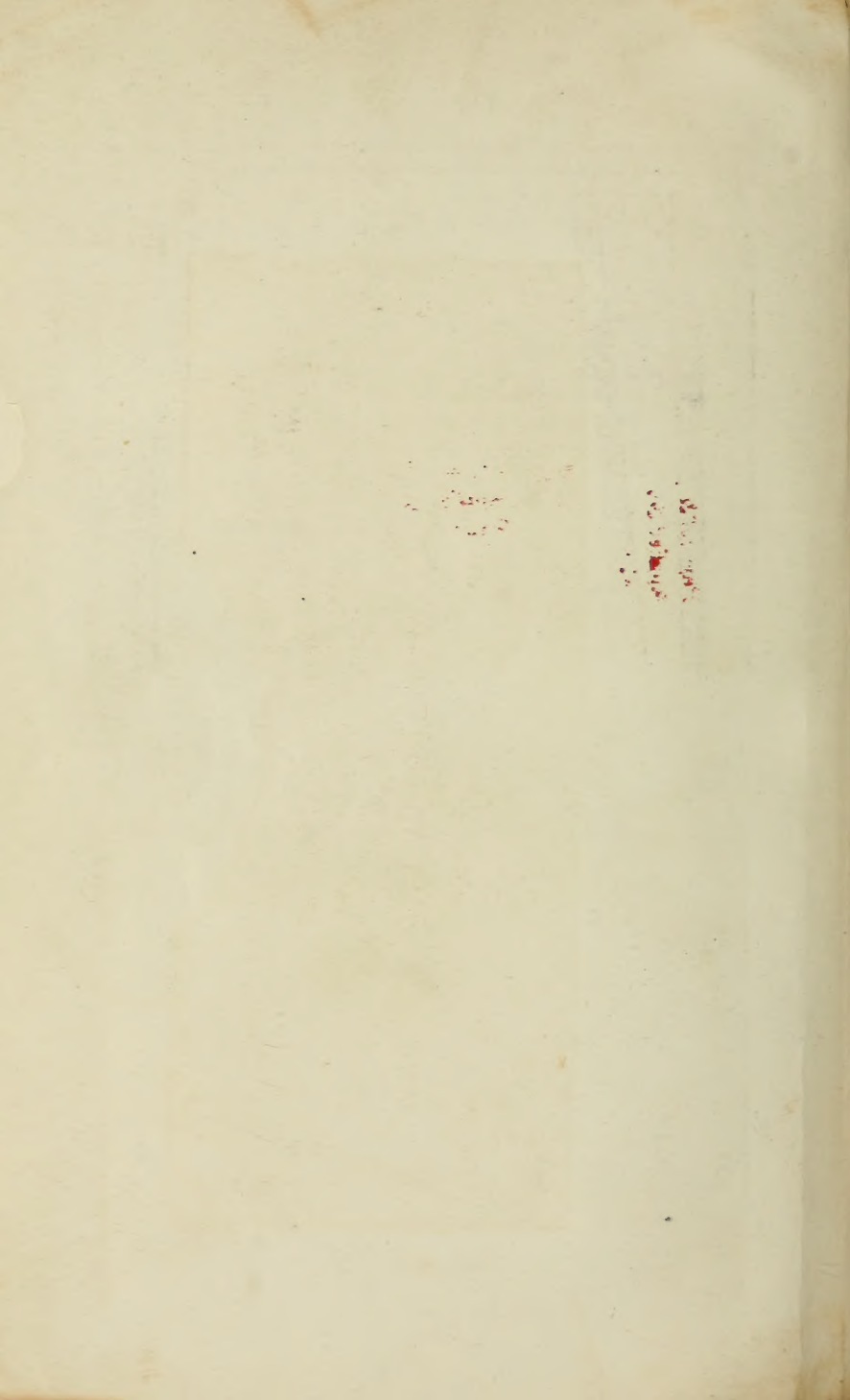
- ACHROMATIC** interference-bands, on, 77, 189.
- Air**, on the change which the internal friction of, experiences at different temperatures and under various pressures, 220.
- Anderson (Prof. R. J.)** on an apparatus illustrating crystal forms, 127.
- Aromatic nucleus**, on the constitution of the, 402, 443.
- Arrhenius (Sv.)** on electrolytic dissociation *versus* hydration, 30; on the influence of solar radiation on the electrical phenomena in the atmosphere of the earth, 75.
- Aubel (E. van)** on the electrical resistance of bismuth, 332.
- Availability**, on endless, 185.
- Biquartz**, on the use of the, in determining the plane of polarization, 134.
- Bismuth**, on the electrical resistance of, 332; on the phosphorescence of, 428.
- Books, new**:—Croll's *Stellar Evolution*, 66; Ridsdale's *Cosmic Evolution*, 67; Casey's *Spherical Trigonometry*, 138; Smith's *Graphics*, 218; Grant Allen's *Force and Energy*, 349; Watts's *Dictionary of Chemistry*, Vol. II., 423; Bernthsen's *Text-book of Organic Chemistry*, 424; Warren's *Elementary Treatise of Mechanics*, 425.
- Bottomley (J. T.)** on the expansion with rise of temperature of wires under pulling stress, 94; on the thermoelectric position of platinum, 163.
- Boys (C. V.)** on quartz as an insulator, 14.
- Burton (Dr. C. V.)** on endless availability, 185; on a physical basis for the theory of errors, 480.
- Callaway (C.)** on the production of secondary minerals in the crystalline rocks of the Malvern Hills, 67.
- Caoutchouc**, on the molecular weight of, 38.
- Carhart (H. S.)** on an improved standard Clark cell, 420.
- Carnot's principle**, on a restriction to the application of, 185.
- Champernowne (A.)** on the volcanic rocks near Totnes, 71.
- Clark cell**, on the, as a source of small constant currents, 353; on the application of the, to the construction of a standard galvanometer, 416; on an improved, with low temperature-coefficient, 420.
- Clayden (A. W.)** on some photographs of lightning and of "black" electric sparks, 92.
- Clouds**, on diffraction-colours with special reference to iridescent, 272.
- Cole (G. A. J.)** on the Northern Slopes of Cader Idris, 69.
- Colloid bodies**, on the molecular weight of some, 38.
- Cooling**, on the law of, 429.
- Copper**, on the spectrum of, 74; on the phosphorescence of, 428.
- Coronæ**, on diffraction-colours, with reference to, 272.
- Crystal forms**, on an apparatus illustrating, 127.
- Currents**, on the Clark cell as a source of small constant, 353.
- Dale (Rev. T. P.)** on a relation existing between the density and refraction of gaseous elements and of some of their compounds, 268.

- D'Arcy (R. F.) on the viscosity of solutions, 221.
- Davidson (C.) on a relation between the sun-spot period and the planetary elements, 426.
- De Heen (P.) on the change which the internal friction of air experiences at different temperatures and under various pressures, 220.
- Diffraction-colours, on, 272.
- Duncan (Dr. L.) on the value of the B.A. unit of resistance in absolute measure, 98.
- Earth, on the origin of movements in the crust of the, 141.
- Electric field, on the magnetic effects produced by motion in the, 1.
- radiation and its concentration by lenses, 48.
- sparks, black, on some photographs of, 92.
- waves, on the propagation of, through wires, 117.
- Electrical phenomena in the atmosphere of the earth, on the influence of solar radiation on, 75.
- resistance of insulators at high temperature, on the, 147.
- of bismuth, on the, 332.
- Electricity, on the passage of, through gases and vapours, 147; on the leakage of negative, caused by sun- and daylight, 427; on steatite as a source of, 428.
- Electrolytic dissociation *versus* hydration, 30.
- Electromagnetic units of electricity, on the ratio of the electrostatic to the, 304, 315.
- Electromotive position of palladium, on the, 351.
- Elster (T.) on the leakage of negative electricity caused by sun- and daylight, 427.
- Errors, on a physical basis for the theory of, 480.
- Fletcher (L. B.) on the ratio of the electrostatic to the electromagnetic units of electricity, 304.
- Fluorescence, on the relation between, and arrangement of molecules, 473.
- Fromme (C.) on the maximum galvanic polarization of platinum electrodes in sulphuric acid, 495.
- Fuller (G.) on a water-spray influence-machine, 42.
- Galvanometer, on the application of the Clark cell to the construction of a standard, 416.
- Gaseous elements and some of their compounds, on a relation existing between the density and refraction of, 268.
- Gases, on the passage of electricity through, 147; on the resistance to disruptive discharge by, under high pressures, 148.
- Geitel (H.) on the leakage of negative electricity caused by sun- and daylight, 427.
- Geological Society, proceedings of the, 71, 139, 492.
- Gladstone (Dr. J. H.) on the molecular weight of caoutchouc and other colloid bodies, 38.
- Glazebrook (R. T.) on the reflexion and refraction of light at the surface of a crystal, 110.
- Gore (Dr. G.) on the molecular constitution of isomeric solutions, 289.
- Gregory (W. G.) on a method of driving tuning-forks electrically, 490.
- Hail, on the theory of, 169.
- Hall (E. H.) on the ratio of the electrostatic to the electromagnetic units of electricity, 304.
- Heat, on the analytical theory of, 429.
- Hertz (Prof. H.) on the propagation of electric waves through wires, 117.
- Hibbert (W.) on the molecular weight of caoutchouc and other colloid bodies, 38.
- Hill (Rev. E.) on the rocks of Alderney and the Casquets, 70.
- Hoffert (Dr. H. H.) on intermittent lightning-flashes, 106.
- Howard (Dr. J. L.) on electric radiation and its concentration by lenses, 48.
- Hull (Prof. E.) on a possible geological origin of terrestrial magnetism, 71.
- Hutchins (C. C.) on metallic spectra, 73.
- Hutchinson (C. T.) on the mercury unit compared with the B.A. unit of resistance, 17; on the value of the B.A. unit of resistance in absolute measure, 98.
- Hydration, on electrolytic dissociation *versus*, 30.

- Influence-machine, on a water-spray, 42.
- Insulators, on the electrical resistance of, at high temperature, 147.
- Interference-bands, on achromatic, 77, 189.
- Iodine, on the molecular condition of dissolved, 146.
- Iron, on the thermal effect due to reversals of magnetization in soft, 207.
- Jennings (A. V.) on the Northern slopes of Cader Idris, 69.
- Kilroe (J. R.) on the origin of movements in the earth's crust, 141.
- Kinetics of bodies in solution, on the, 145.
- Klatt (V.) on the phosphorescence of copper, manganese, and bismuth in the sulphides of the alkaline earths, 428.
- Koller (Dr. H.) on the electrical resistance of insulators at high temperature, 147.
- Kristensen (K. S.) on Leidenfrost's phenomenon, 220.
- Krug (A.) on the energy of thermal radiation at a white heat, 352.
- Lees (C. H.) on the law of cooling and its bearings on certain equations in the analytical theory of heat, 429.
- Leidenfrost's phenomenon, on, 220.
- Lenard (P.) on the phosphorescence of copper, bismuth, and manganese in the sulphides of the alkaline earths, 428.
- Light, on the reflexion and refraction of, at the surface of a crystal, 110; on the mechanical equivalent of, 352.
- Lightning, on some photographs of, 92; on intermittent flashes of, 106; on, and gunpowder magazines, 368.
- Lodge (Prof. O. J.) on electric radiation and its concentration by lenses, 48.
- Loeb (M.) on the molecular condition of dissolved iodine, 146.
- Luminosity, on the mechanics of, 149, 248, 376.
- Lyons (H. G.) on the Bagshot beds, 144.
- McConnel (J. C.) on diffraction-colours, 272.
- Maclean (M.) on geometrical optics, 400.
- McMahon (Major-Gen.) on the rocks of the Lizard district, 139.
- Magnetic effects produced by motion in the electric field, on the, 1.
- Magnetism, on a possible geological origin of terrestrial, 71.
- Magnetization, on the thermal effect due to reversals of, in soft iron, 207.
- Manganese, on the phosphorescence of, 428.
- Mentzner (M.) on steatite as a source of electricity, 428.
- Metallic spectra, on, 73.
- Motion, on the magnetic effects produced by, in the electric field, 1.
- Natterer (Dr.) on the passage of electricity through gases and vapours, 147.
- Nernst (W.) on the kinetics of bodies in solution, 145.
- Optics, on geometrical, 232, 400.
- Palladium, on the dependence of the electromotive position of, on the quantity of hydrogen it contains, 351.
- Pickering (S. U.) on the nature of solutions, 148.
- Platinoid, on the thermoelectric position of, 163.
- Platinum electrodes, on the polarization of, 495.
- Polarization, on the use of the bi-quartz in determining the plane of, 134.
- Pollock (A.) on the Clark cell as a source of small constant currents, 353; on the resistance of imperfectly purified sulphur, 469.
- Prestwich (Prof. J.) on the Pebbly Sands of Suffolk, 142.
- Quartz as an insulator, on, 14.
- Radiation, on electric, and its concentration by lenses, 48; on the energy of thermal, at a white heat, 352.
- Rayleigh (Lord) on achromatic interference-bands, 77, 189.
- Resistance, comparison of the mercury unit with the B.A. unit of, 17; on the value of the B.A. unit of, in absolute measure, 98; on the measurement of high specific, 452.
- Roberts (T.) on the Upper Jurassic clays of Lincolnshire, 140.
- Rosa (E. B.) on the ratio of the elec-

- tromagnetic to the electrostatic unit, 315.
- Rowland (Prof. H. A.) on the ratio of the electrostatic to the electromagnetic units of electricity, 304.
- Rücker (Prof.) on the relation existing between the density and refraction of gaseous elements, 268.
- Rutley (F.) on tachylyte from Whiteinch, 143.
- Shunt-transformer, on a, 132.
- Smith (E. W.) on a shunt-transformer, 132.
- Solar radiation, on the influence of, on the electrical phenomena in the atmosphere of the earth, 75.
- Solutions, on the nature of, 148; on the viscosity of, 221; on the molecular constitution of isomeric, 289.
- Spectra, on metallic, 73.
- Steatite as a source of electricity, on, 428.
- Sulphur, on the resistance of imperfectly purified, 469.
- Sun-spot period, on a relation between the, and the planetary elements, 426.
- Sworn (S. A.) on the constitution of the aromatic nucleus, 402, 443.
- Tanakadaté (A.) on the thermoelectric position of platinoid, 163; on the thermal effect due to reversals of magnetization in soft iron, 207.
- Thermodynamics, on the second law of, 493.
- Thoma (Max) on the dependence of the electromotive position of palladium on the quantity of hydrogen it contains, 351.
- Thompson (Prof. S. P.) on geometrical optics, 232.
- Thomson (J. J.) on the magnetic effects produced by motion in the electric field, 1.
- Threlfall (Prof. R.) on the Clark cell as a source of small constant currents, 353; on the application of the Clark cell to the construction of a standard galvanometer, 416; on the measurement of high specific resistance, 452; on the resistance of imperfectly purified sulphur, 469.
- Tomlinson (C.) on the theory of hail, 169; on lightning and gunpowder magazines, 308.
- Tumlirz (O.) on the energy of thermal radiation at a white heat, 352; on the mechanical equivalent of light, 352.
- Tuning-forks, on a method of driving, electrically, 490.
- Viscosity of solutions, on the, 221.
- Ward (A. W.) on the use of the bi-quartz in determining the plane of polarization, 134.
- Wiedemann (Prof. E.) on the mechanics of luminosity, 149, 248, 376; on the second law of thermodynamics, 493.
- Wilkes (G.) on a comparison of the mercury unit with the B.A. unit of resistance, 17; on the value of the B.A. unit of resistance in absolute measure, 98.
- Wires, on the expansion with rise of temperature of, under pulling stress, 94; on the propagation of electric waves through, 117.
- Wolf (M.) on the resistance to disruptive discharge by gases under high pressures, 148.
- Zinc, on the spectrum of, 74.

END OF THE TWENTY-EIGHTH VOLUME.



QC

The Philosophical magazine

1

P4

ser.5

v.28

Physical &
Applied Sci.
Serials

PLEASE DO NOT REMOVE
CARDS OR SLIPS FROM THIS POCKET

UNIVERSITY OF TORONTO LIBRARY

